

Volume 66 Number 4

August 1961

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Published six times a year by the  
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Printed six times a year by George Banta Company, Inc., Curtis Reed Plant, Menasha, Wisconsin, U.S.A. during the months of February, April, June, August, October and December, for the American Anthropological Association. Second class postage paid at Menasha, Wisconsin. Accorded the special rate of postage provided for in paragraph 4, section 538, F.L.H.R. authorized August 23, 1932.

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VOLUME 63

*Printed in*

MENASHA, WISCONSIN, U.S.A.

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1961

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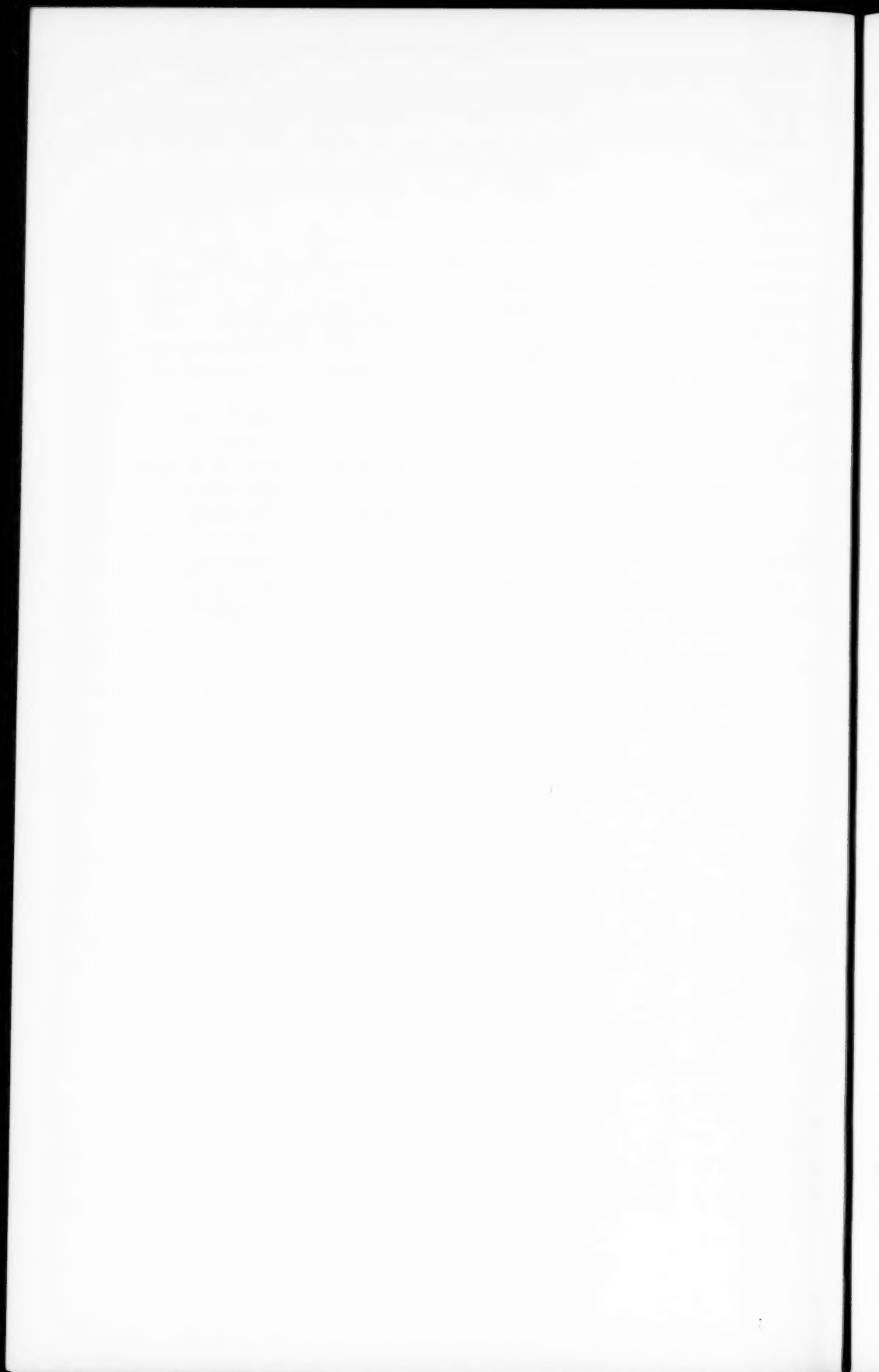
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## *Ideology and Culture in Uganda Nationalism<sup>1</sup>*

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### I

**N**ATIONALISM is an ideological commitment to the pursuit of the unity, independence, and interests of a people who conceive of themselves as forming a community. It may aim at uniting a community divided by political boundaries, or it may pursue the independence of a community which is incorporated within some larger political entity. In most parts of Africa, nationalism and nationalists have faced a difficult dilemma: What is the unit to which nationalist ideology should be directed? On the one hand, the colonial territories into which the continent was divided during the 19th century usually have little community of language or culture. Although these territories form the organizational matrix for the formation of political movements aimed at independence—and must necessarily do so because they are the organizational base of the power which nationalists hope to displace—still, these territories as such hold little permanent inspiration for Africans beyond opposition to the colonial powers which created them. There is little that could make up the ingredients of a Nigerian or Congolese or Tanganyikan “personality.” On the other hand, the indigenous communities of Africa—the tribes—are obviously unsatisfactory bases for modern nationhood. They are usually too small and their institutions are too inadequate to the tasks of a modern state—in fact are often incompatible with these tasks. African leaders, however, want both community and viable political units; they want both the loyalty and consensus which will induce their people to support, or at least acquiesce in, the difficult tasks which lie ahead, but they also want states large enough and homogeneous enough to be capable of responding to these tasks. These problems, of course, exist in Asia as well, but the greater indigenous heterogeneity of Africa makes them particularly difficult there.

Now, in one of its aspects, this is a cultural problem. There are also, of course, organizational problems in the emergence of new nations in Africa—problems concerning the arrangement of persons and groups—but we should like to draw attention here to the cultural aspect of the process—the aspect which is concerned with the emergence of new patterns of value and belief. The new nations, in order to achieve a degree of unity of purpose, need cultures which, first, will provide a measure of consensus among their diverse peoples and, second, will be capable of the constant innovation which existence in the modern world requires. It is perhaps not an abuse of words to speak of this as a problem in “cultural management” and to use the term “ideology” to refer to that part of culture which is actively and explicitly concerned with the

establishment and defense of patterns of value and belief. Ideology is thus the apologetic part of culture. If we look at it in this way, those whose task it is to create cultures for the new nations—the ideologists of nationalism—have two major sources to draw upon: on the one hand, traditional African cultures and, on the other, the many and diverse elements of value and belief which may be imported from modern Europe, Asia, and America. The task is a difficult one and fraught with dilemmas: how to create in Africans a sense of self-esteem without encouraging tribalism; how to be “modern” without being “Western”; how to change rapidly without losing a sense of continuity and cultural “wholeness.”

In Uganda, the territory which we shall discuss, these dilemmas are very apparent. Uganda is a British protectorate, the core of which is formed by the Kingdom of Buganda. Buganda is the core in the sense that the political arrangements upon which the Protectorate rests were originally entered into in the 1880's and '90's by the British and the Baganda and, only later, and in most cases with considerably less consent, were extended to the penumbra of tribes which now make up the Eastern, Western, and Northern Provinces. A good deal of this added territory was, in fact, conquered jointly by the British and the Baganda. Buganda is also the core of the country in another, and more contemporary, sense. Buganda has contained both the capital and the commercial center of the Protectorate and consequently the new political and economic institutions which have grown up during the association with Great Britain have their locus in Buganda.

Now in Buganda, which because of its central position in the country greatly influences the quality of its politics, there has developed a peculiar combination of receptivity to innovation—a receptivity which in itself is a great asset to a nation attempting to modernize—together with a very profound commitment to certain traditional institutions and to the independent identity of Buganda—a commitment which militates against the development of a Uganda-wide unity. The Baganda have enthusiastically embraced Western education, Christianity, and a whole range of modern techniques and skills. They have, in fact, developed a deep ideological commitment to modernity—to progress—have made these things part of their culture to such an extent that Western observers ever since the turn of the century have called them “the most progressive people in eastern Africa,” “the Japanese of Africa,” etc. At the same time, they have retained such a deep sense of cultural identity and integrity that they are the despair of both colonial administrators and non-Baganda political leaders, each in their own way interested in territory-wide unity. Thus, they also appear to be the least assimilable people in eastern Africa. The Baganda have replaced huge chunks of their traditional culture with Western elements and yet are able to feel that their present culture has unity and is essentially theirs.

It is this paradox of profound change combined with profound separatism and conservatism (of a sort) which is at the heart of the Uganda version of the African nationalist's dilemma and which we want to try to explain. The prob-

lem may be stated thus: How has it happened that in Uganda the people who are ecologically at the center of modern economic and political development and who are ideologically most committed to modernization are at the same time the least prepared to subordinate their identity to a wider Uganda nationhood? First, we may survey briefly what we may call the acculturation situation in Uganda—try to indicate how much of Kiganda culture has changed as compared with the cultures of neighboring peoples. Then we must try to say something about the Kiganda ideology of culture change—the way the Baganda think about and justify their new culture. In doing this we shall have to say something about the historical context in which this ideology developed.

## II

We may look first at the traditional state and the changes which it has undergone. Traditional Buganda was a despotism—a polity in which the independent legitimate authority of the monarch, the *Kabaka*, was maximized. At some time in the past, according to legend, the most important political offices were held by the heads of, and on behalf of, patrilineal clans and lineages. In more recent times, unilineal descent groups continued, as they still do today, to govern inheritance and succession in the domestic sphere, but more and more the government had passed out of their hands and into the hands of chiefs who were appointed by a *Kabaka* and loyal to him alone—a kind of proto-bureaucracy. We know something of how this happened, for the process was still going on in the late 19th century, a period for which we have documentation. However, it is sufficient to say that by the late 19th century, the kingdom was ruled by a corps of chiefs who owed their positions to the personal grace of the *Kabaka*, who might appoint, dismiss, transfer, and even execute them at will. Every able young man aspired to a career in the service of the *Kabaka*, around whom was woven a cultural tissue of arbitrary fierceness and power. The *Kabaka* was “the lion,” “the queen ant who feeds upon her subjects,” the “charcoal fire in which the nation is forged.” A chief might one day be at the pinnacle of wealth and influence—the ruler of a large district and the recipient of lavish tribute from his people and estates from the *Kabaka*; next day, having incurred the monarch’s disfavor, he might be stripped of property and office, lying in stocks, the object of scorn and physical tortures of a most imaginative kind. As we know, absolute power does not exist; there were checks in the form of advice from friends and kinsmen, but these were minimal. Consent was secured, not so much by constraint upon the *Kabaka* as by a nation-wide psychic and material pay-off in the shape of the spoils of war. The whole kingdom in the 19th century had become organized for expansion and plunder at the expense of neighboring peoples.

Today there is still a *Kabaka*’s Government, recognized by the British Protectorate Government and granted a wide measure of autonomous authority. The material manifestations of this government and the activities of its members have been transformed. It occupies a large and magnificent new

building at one end of a wide, tree-lined boulevard, at the other end of which is the Kabaka's modern palace. Inside the building are the most modern offices, with typewriters, filing cabinets, and telephones, occupied by the Ministries of the Treasury, Justice, Education, Health, and National Resources. In another part of the building is a legislative hall in which sits the modern Buganda parliament, a largely elected body. In the districts outside the capital there are, again, offices manned by functionaries employed on civil service terms and engaged in the tasks associated with modern government. The Kabaka himself is an elegantly-tailored, Cambridge-educated young gentleman who speaks flawless English. Along with all this change, there is also continuity: The government building is called the *Bulange* after the traditional audience-hall of the Kabaka; the parliament is called the *Lukiiko* after the traditional gathering of the chiefs to pay homage; the administrator in charge of the District of Buruli carries the title of the *Kimbugwe*, the traditional Keeper of the King's Umbilicus, which has great ritual significance. A man appointed, say, senior assistant to the Minister of Health, appears before the Kabaka, in a well-tailored business suit, and performs a war-dance, holding an imaginary spear, and recites, as did his ancestors: "I am a Muganda, a man of the Kabaka; my totem is the lung-fish and my great-great-grandfather was Serukenya of the lineage of Nalimanga, who sired Makino, who sired Sebbanja, who was my father." He then throws himself to the floor, new suit and all, and begs the Kabaka to accept his thanks. Perhaps most important of all, the doings of the government are reported and discussed in some half-dozen substantial newspapers printed in Luganda—certainly one of the very few really vigorous native-language presses in Africa.

In the field of religion, too, there has been tremendous change, with overtones of continuity. Traditional religion centered upon the cults of gods, ancestors, and nature spirits. Communication through spirit possession with these beings, all of whom were capable of influencing man's life on earth, was the principal form of religious activity. Ordinary persons sought communication with the spirits in order to cure a disorder, to secure children, or to further their political careers. The Kabaka sought it on behalf of the nation, particularly to secure supernatural support in war.

Today most of this has passed away and the vast majority of Baganda are Anglican or Roman Catholic Christians, with a strong minority of Sunni Muslims. The Christian churches are all but legally established. The Kabaka is crowned in the Anglican cathedral and there is an agreement whereby major posts in the government are shared out among the three religious groups. Both Anglican and Roman Catholic churches have largely Baganda clergies, including bishops. There is no doubt at all that the churches have entered deeply in Baganda life—have become Kiganda churches. Most importantly, perhaps, the leading members of society, in a traditional sense, are pillars of the church. Chiefs and wealthy landowners often have reserved for them special pews in little country churches which they have built. There are, of course, thousands of merely "nominal" Christians, just as there are in Europe and America, and

there is a similar secularizing tendency, but an objective observer would probably say that, while Baganda are typically tempted, and sin—in rather different directions from Westerners because they live in a different milieu—probably, on the whole, they sin no more frequently. For example, there are important remnants of the past in the shape of a social system which encourages polygyny and a tradition of “pagan” magic, and Baganda often yield to both. This, however, only makes the Baganda more exotic sinners than Westerners, not more frequent or more profound sinners, for they remain Christians in a real sense all the while.

Closely associated with religion in the field of cultural change is education, for the missions which brought Christianity also founded Western-type schools. The response to Western education—and to technical training of all kinds—has been enthusiastic from the very beginning. The problem for the missions and for the government has always been to provide sufficient schools to satisfy the demand. Again, the leading members of society—the Kabaka and his leading officials—are moderately-to-well educated and they make great efforts to see that their children are even better educated. Furthermore, a good many of the elite have for half a century been educated in the very type of institution best calculated to produce the maximum socialization impact—the English “public school” type of boarding school, after which the Baganda schools were modeled. The leading Buganda government officials and the leaders of the most important political parties are overwhelmingly “old boys” of the two elite boarding schools—one Anglican and the other Roman Catholic.

Finally, there has been a radical transformation of the economy. In traditional Buganda, the average family was engaged in subsistence agriculture, growing plantains for its staple food and perhaps producing a specialized handicraft product—pottery or barkcloth or iron hoes—for exchange with its neighbors at the weekly market. Most circulation of goods and services was incidental to the exercise of political authority; that is to say, jurisdiction over land and people were one and the political hierarchy was supported by taxes and tribute in goods and services levied upon the ordinary people.

Today the average Muganda is a cash-crop farmer growing cotton or coffee for money with which to buy an ever-expanding range of imported goods. Often he holds freehold title to his land and he very frequently hires migrant foreign workers from Belgian Ruanda-Urundi by the day to supplement the family labor force during the busy season. He responds to the market in much the same way as a midwestern American farmer and tends to be coldly instrumental in his approach to agricultural technique. For example, during the political crisis of 1953–55, when the Kabaka had been exiled by the Protectorate government and when relations between the Baganda and British political officers were extremely bad, British officers of the Agriculture Department reported practically no disruption of their normally good relations with farmers. Agriculture, the Baganda felt, was a matter which should not be confused with politics.

There are more subtle levels of cultural change at which it is possible here

only to hint. One of the striking characteristics of Baganda is their ability to wear Western clothing with a real feeling for style. Over much of Africa, Western clothing is worn like an uncomfortable, ill-fitting uniform, but Baganda men and women have penetrated sufficiently into the inner recesses of Western style that many of them can wear Western clothes with real taste.

### III

So: Baganda are, in many ways, extremely "acculturated" and the leading members of society are the most acculturated of all. There are here no culturally conservative, traditional chiefs pitted against a group of young, Western-educated, commoner politicians. Baganda do not see, or practice, politics in these terms, as so many African peoples do. Rather, Kiganda society has acculturated, as it were, from the top down and hence the new culture tends to have universal legitimacy. Indeed, from the point of view of the Baganda, this new culture, which includes many Western ideas of government, Western education, Anglican and Roman Catholic Christianity, the motivations appropriate to a money economy—all this has become *their* culture in a fundamental way. They have, so to speak, "naturalized" the foreign elements and thus kept a sense of cultural integrity and "wholeness" through a period of radical change. They have, furthermore, developed an ideology to account for all this—an ideology which allows them to absorb foreign elements wholesale without loss of self-esteem, without loss of their deep feeling of "Ganda-ness."

This ideology is expressed in the remarkable body of Luganda literature which the Baganda have produced since they became literate at the end of the last century, a literature which includes histories, memoirs, biographies, novels, and political pamphlets. Brief quotations from a few representative works will illustrate the ideology to which we wish to draw attention.

First, an Anglican clergyman, who introduces his history of Buganda with an allegorical summary comparing the history of the kingdom with the life of an individual. Buganda, he says, was born under Kabaka Kintu, the legendary first king; went to school under Kabaka Mutesa, who was king when the first Europeans arrived in the 1860's; and came to maturity under Mutesa's son, Mwanga:

It was Mutesa who taught the child Buganda its future. . . . He called the Europeans to come and teach his people . . . the higher school in which he taught them was summoning the Christian missions to come to Uganda and ordering his people to become Christians (Zimbe 1939).

Then we may turn to a politician and newspaper editor, who has written two successful novels (Mulira n.d.; 1951). Both are "young man makes good" stories and in both cases "making good" consists in being "progressive" in the sense of learning things Western. The novelist clearly distinguishes between Western people and Western culture. In *Aligaveesa* (the title is the name of the hero), the career of the young man is blocked by racial discrimination but he continues to pursue "progressivism" anyhow.

Lest it be thought that the ideology we refer to is confined to a group of "Uncle Toms" subservient to Europeans, let us cite a passage from a political pamphlet which was banned as seditious and which British officials regard as the most bitterly anti-European piece of writing ever published in Uganda. In a pamphlet entitled *Buganda Our Mother*, which carries roughly the chauvinistic emotional overtones of Hitler's phrase "blood and soil," the author accuses Europeans of trying to steal the land of Buganda and enslave its people. Nevertheless, he, too, is concerned to begin his work with the assurance that he regards the acceptance of elements of European culture as a good and necessary thing. The first British governor, he says, introduced the freehold land system:

. . . intending to preserve the ties of great friendship entered into by *Kabaka Mutesa* with Her Majesty the Queen of the great Kingdom of England. . . We were greatly helped by the European missionaries who had been invited by *Kabaka Mutesa* to come and preach the Gospel of the Light of Truth (*Mukubira n.d.*).

The theme is made most explicit in the work of the greatest Muganda ideologist of them all, the late *Kabaka Daudi Cwa*, the father of the present *Kabaka*, who wrote a number of pamphlets for the guidance of his people. In a most interesting one entitled *Education, Civilization and Foreignization in Buganda*, he says:

. . . I have considered it my duty to warn very strongly all members of this young generation of Baganda that while they are legitimately entitled to strive to acquire education and civilization, they should also take very great care that acquisition of Western education and civilization do not destroy their best native traditions and customs, which in my opinion are quite as good as those found among Western civilized countries but which only require developing and remodeling where necessary . . . (*Daudi Cwa 1947*).

If we were to synthesize what these people are saying, it would go something like this: Traditional Buganda was a progressive and vigorous country which, unfortunately, had not had the opportunity of becoming Christian and modern but which was ready to respond to these things when they were offered. *Kabaka Mutesa* recognized this and invited the Europeans to come and teach the Baganda. The "deal" was thus entirely voluntary on Buganda's part. When the Europeans have finished teaching what they know, they should leave. There is nothing basically incompatible between Kiganda culture and the "useful" parts of European culture; on the contrary, these European elements should be absorbed into what remains a basically Kiganda way of life.

This is the ideology by which the Baganda have lived and by which they have tried to regulate their dealings with Europeans. It has allowed them to be both intensely chauvinistic and remarkably progressive. The combination is a somewhat unusual one, and in order to explain how it was achieved we must refer to the structure of traditional Kiganda society and the way it reacted to Western intrusion. This is necessary because, as *Max Weber* has taught us, ideology does not work by itself; ideologies only develop and become dominant when there are groups within society capable of, and interested in, promoting them.

## IV

The creators and carriers of the new ideology in Buganda were the young chiefs who achieved high office under the Kabaka in the 1880's and '90's. We may recall that in traditional Buganda, at any rate by the 19th century, most of the important political offices had ceased to be hereditary. Most were appointed by the Kabaka and the most important recruiting-ground for chiefs was the corps of pages—young boys who were sent to the palace to serve the Kabaka. A quick-witted boy would catch his master's eye and be given a subordinate post—the first step toward the higher reaches of power if he continued to serve loyally and intelligently. Now, in comparative perspective, this was, for Africa, an unusually mobile and open type of political hierarchy. It is much more common in African kingdoms for there to be an important hereditary element in political recruitment. The difference in Buganda is important for our purposes, for it meant that *within the traditional system* there was room for the wholesale replacement of personnel. An entirely new group of men could take power without violating the system, and this is what happened.

First the Arab traders and then the Christian missionaries established themselves at court and began to proselytize among the pages. Why traditional Kiganda religion did not oppose this with greater vigor is a question which we need not pursue; we may say simply that the Kabaka regarded all religion in an essentially instrumental way and hence encouraged competition among Christianity, Islam, and the traditional cults in the hope of deriving maximum benefit from all of them without committing himself to any. The result was a struggle among factions of young chiefs, aided by the Arabs in the case of the Muslims and the Europeans in the case of the Christians. The Christians won.

Thus it happened that a new set of chiefs, committed to Christianity and progress, came to power at a time when the fundamentals of the relationship between the British and the Baganda were being worked out. Because they had been recruited from traditional sources and because the traditional system provided for recruitment through achievement, they did not look upon themselves as revolutionaries in the sense of basically altering political arrangements; rather they were Christianizing and improving a king and kingdom to which they remained intensely loyal. (We know something about how they saw these events because several of them wrote memoirs.) Although they were at times made aware of British power in a way which indicated quite clearly that the protectorate was not really an arrangement arrived at between equals, still they could believe that they had secured the benefits of association with Britain with a minimum of political interference. There was much rancor in later years between the two parties over the interpretation of the Agreement which set the terms of relationship, but the Baganda have always held, with substantial success, to their version, elaborated in the ideology which has been described.

Thus it happens that the Baganda, the pivotal tribe in Uganda, have contributed to the country a powerful impulse toward modernization but have also



remained intensely chauvinistic and reluctant to merge themselves into a wider Uganda nationalism. The other peoples of Uganda tend to regard the Baganda with a mixture of admiration and resentment. Among their Bantu-speaking cousins in southern Uganda, admiration tends to predominate, while among the Nilotic—and so-called “Nilo-Hamitic”—speaking peoples of the north, resentment is perhaps the stronger element. Everywhere, however, there is reluctance to solve the problem by accepting Baganda domination, for the Baganda, in their peculiar form of “progressive isolationism,” have been emulated by the other tribes during most of the past fifty years with the active encouragement of the British Protectorate Government. The Baganda, it would seem, must either seek their political future alone or else be prepared to give up the unique ideological advantages of their isolation.

## V

The situation which we have described is an unusual one in Africa. In most parts of the continent there has been less continuity between traditional and modern culture, either because colonial regimes have actively promoted discontinuity, as in the French territories, or because traditional societies have been too inflexible to permit the rise to positions of influence within traditional society of the bearers of the new cultural syntheses—a situation common in British areas even where continuity has been favored. But if this case is unusual, the broader problem of “cultural management” is not. Everywhere there is the need to create cultural community where in the past little existed. African ideologists lack the cultural resources which their Asian counterparts find in the religious “high cultures” of Hinduism, Buddhism, and Islam. (Many Africans, of course, are Muslims, but few new African nations are even largely Muslim.) The more general, underlying cultural unities of traditional Africa, or even of those regions which form the new national states, though undoubtedly real on some level, are as yet too vaguely understood to make good ideological material. (By contributing to an understanding of these unities anthropologists may, indeed, contribute in an important way to nation-building!) Neither do the cultures of the metropolises—France, Britain, Belgium, and Portugal—provide satisfactory sources for new national cultures (though of course they contribute to them), if for no other reason than that these have for too long been identified with negrophobia. The French African intellectuals who have so whole-heartedly identified themselves with French culture can find in it a real source of self-esteem only by reversing its judgment of themselves. Their slogan, “negritude,” tends to be little more than a call for equality, or dominance, for dark-skinned Frenchmen—a thin, and even dangerous, basis for a national culture.

Our purpose, however, is not so much to relate the difficulties faced by modern Africa's ideologists; rather we wish to draw attention to the burst of cultural creativity which these difficulties will undoubtedly call forth. Far from robbing anthropologists of their material, the “winds of change” in Africa may provide them with an unprecedented opportunity to study culture in the making.

## NOTE

<sup>1</sup> Material for this paper was gathered during 1953-57 while the writer was a member of the East African Institute of Social Research, working under a generous grant from the Carnegie Corporation of New York. The Carnegie Corporation is of course in no way responsible for its contents. An outline of traditional and modern culture and society in Buganda and a bibliography of the relevant literature will be found in Fallers (1960).

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## Food Transport and the Origin of Hominid Bipedalism<sup>1</sup>

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MAN'S upright posture and bipedal gait have been justly considered outstanding features of human nature, for example by Haeckel (1868), Darwin (1871), Engels (1876), Munro (1893), Smith (1913), Hooton (1925), Jones (1926), Freud (1930), Weidenreich (1946, 1947), Keith (1949), Schultz (1950, 1951), Washburn (1951, 1959), Mayr (1951), LaBarre (1954), W. E. L. Clark (1955, 1959), Piveteau (1957), Delmas (1958), Vallois (1958), Howells (1959), and Dart (1959a). For Engels in 1876, erect posture was "the decisive step in the transition from ape to man," an idea he shared with Haeckel (1868, 1906 ed.:405) who said that upright posture was the first of the "three great processes in the development of the human organism." For Hooton (1925), man became man when he definitely took to the ground and assumed upright posture and bipedal progression. Freud (1930:66n) wrote that "Man's erect posture . . . would represent the beginning of the momentous process of cultural evolution." Schultz (1951:38) asserted that the acquisition of upright posture was the "first major step in man's evolution." Mednick, studying the evolution of the human ilium (1955), states that "the evolution of bipedal locomotion seems to have preceded other uniquely human attributes. It appears quite probable that our ancestors walked first, and subsequently became large-brained, tool-using humans." W.E.L. Clark (1955:13-14) believes that bipedalism was the most important single factor in Hominid evolution. Piveteau (1957:278) cites the adoption of upright posture as "the decisive disposition for the individualization of the human branch," with Pongid and Hominid divergences having their point of departure chiefly in modifications of the pelvis (p. 307). Washburn, who has dealt in detail with the locomotor changes which apparently initiated hominization, considers bipedalism the basic adaptation for the foundation of the human radiation (1951:68-70), starting man on "his separate evolutionary path" (1959:31), though tool-using may have been almost as early (1960a) or simultaneous (Washburn and Howell 1960:35-37, 47). DuBrul (1958:90) notes that upright posture is essentially a "reduction of the repetition of structures serving the same function," with the forelimbs becoming "as it were, accessory mandibles rather than locomotor devices," leading to a "new mode of feeding and feeding niche." Mayr (1951) says that upright posture led the protohominids into a "completely different adaptive zone" where new and severely increased selective pressures developed.

If these ideas are valid, it is extremely important to show not only what changes occurred in the pelvis and limbs of the evolving protohominids, but *why* they occurred—i.e., to determine what conditions would make such an

anatomical transformation biologically advantageous. This paper is an effort to set forth a reasonable and sufficient explanation which has the further merit of being experimentally testable, within limits, using infrahuman Primate subjects.

Bipedalism is of course not limited to man (cf. Reynolds 1931). Birds are bipeds, and penguins stand in a fully vertical posture. Many Mesozoic reptiles were bipedal. Certain marsupials and rodents are bipedal and, among the carnivores, bears are well known for their occasional upright stance and locomotion, which happens to be quite manlike. Valuable comparative work has been done on non-Primate bipedalism (cf. DuBrul 1950), but none of these studies indicates why regular bipedalism came about in the ancestry of mankind.

The bipedal proclivities of many infrahuman Primates are well known. Such behavior is spontaneous and not dependent upon human training or example (Riesen and Kinder 1952; Weidenreich 1947:223; Schultz 1950, 1951:3; Keith 1912; Darwin 1871; Haeckel 1900; Smith 1913). Even lemurs are occasionally bipedal, notably sifakas and indris. The highly arboreal New World monkeys are rarely bipedal, though the capuchin is readily trained to this gait. Among Old World monkeys, bipedalism is spontaneous though infrequent in macaques. A juvenile stump-tail macaque being studied at the University of Colorado walks and stands upright with ease and without training. The stance and gait are bent-kneed (cf. Washburn 1951:71, who goes into valuable detail on muscular changes involved in the assumption of fully human bipedalism). Among the great apes, bipedal proficiency varies from the rare upright stance in orangs to the easy biped walk or run of gibbons, but also on an individual basis. Dart (1959a:218) quotes a case of a Munich zoo orang who learned to walk and stand upright, with the soles of his feet flat rather than curled inward like the ordinary members of his species. Köhler noted that some of his chimpanzees, "whose peculiar build is well adapted to the upright posture," would adopt bipedalism as a fashion or form of play for several days at a time (1959:279). I have observed easy bipedal walking in one of the subadult gorillas at the Cheyenne Mountain Zoo in Colorado Springs. A newly captured male gorilla living under semi-natural conditions in the former Belgian Congo has been photographed striding bipedally with a large bundle of leaves under one arm (*Life* magazine 1960), and also running upright while beating his chest in the manner first described by Du Chaillu. Schultz (1951:38) concludes that the ability to walk bipedally is "unmistakably present in apes, but perfected only in man." Whatever the reasons for this behavioral alternative to quadrupedalism among the infrahuman Primates may be, we may consider this *ability* to walk bipedally as a given, antecedent to the evolutionary emergence of the Hominidae. It is essential to our argument that many, if not most, Primates can walk bipedally if need be, but also essential to remember that no nonhuman Primate *regularly and habitually* uses bipedal locomotion. This is what has to be explained, and not the given anatomical capacity to walk or stand upright occasionally.

Our intention is not to examine the many interesting side-effects of bipedal-

ity and vertical posture on human anatomy and physiology, some of which have left scars well known to the medical profession (Hooton 1946; Keith 1912; Krogman 1951; Martin and Saller 1958:1006; DuBrul 1958:38). The persistence of inguinal hernia, intervertebral disc difficulties, various circulatory defects, etc., suggest that Schultz (1951:58) was optimistic in stating that bipedalism had been perfected in man. More to the point of this paper, these defective adaptations suggest that natural selection for bipedalism among the emergent hominids must have been rapid and ruthless.

Thanks to recent work on fossil Australopithecinae, we are sure that the achievement of upright posture and gait came well before the expansion of the braincase and reduction of the snout to human proportions (W.E.L. Clark 1954a; Howell 1955; Spuhler 1959; Washburn 1959; Etkin 1954:132; Piveteau 1957:315; Dart 1958b, 1959a). Only a few decades ago, the prime mover of hominization was seen to be the expanding cerebral cortex (Smith 1913; Teilhard de Chardin 1958:157). As late as 1948, Kroeber could attribute the evolution of man's capacity for culture almost exclusively to cortical change (pp. 68-71). To be sure, culture in its later manifestations would have been impossible without an enlarged brain, but such a brain now seems to have been only one of the responses to new living conditions made possible by the shift to regular bipedal locomotion (Shapiro 1956:19). Indeed, Piveteau (1957:65) mentions a mechanistic hypothesis for the expansion of the prehomimid brain, as a direct consequence of the attainment of the erect posture. With the head balanced on top of the spine, the heavy neck muscles would be reduced; this in turn would permit the occipital expansion of the skull, accompanied by lessened muscular involvement of the temporals arising from reduced jaw and tooth size. Piveteau notes that this theory reverses the relation of brain to braincase, and ignores the fossil evidence of rather heavy neck musculature in Neanderthal man with his large braincase.

Stated simply, our problem is: why did certain quadrupedal Primates, already capable like other Primates of sporadic bipedalism, become habitual bipeds? (Cf. Vallois 1955:2176-77; Washburn and Howell 1960:47.) Our suggested answer, to be elaborated below, is: because the effective use of a new food resource required its transport over considerable distances, and only bipedal locomotion, *by freeing the arms and hands for carrying*, could achieve maximal transportational efficiency. Obviously we are dealing with a time before the invention of pack-straps and pack-saddles—cultural solutions to animal transport which permit even hoofed quadrupeds to carry heavy loads for long distances.

Mendes-Corrêa (1924) once divided theories of anthropogenesis into those stressing external (exogenic) conditions, such as changes of habitat or food, and those emphasizing internal (endogenic) conditions, such as changes in the brain, sexual, and social behavior. Theories accounting for hominid bipedalism can be similarly divided. Thus, Elliot Smith laid great emphasis on the brain, vision, and hand and eye coordination, with erect posture a by-product of the growing importance of hand-eye coordination as the hand is freed from loco-

motor tasks (1913). Kroeber (1948:68) said that with capable-enough brains, chimpanzees could have achieved culture without ever becoming bipeds. Hayes and Hayes (1954) agreed with this on the basis of their chimpanzee research, claiming that "erect posture was a secondary character of only moderate importance." Straus (1953) dissents most vigorously. On this planet, at least, brains capable of carrying on culture have only developed in bipeds. Science fiction presents numerous and ingenious examples of culture-carrying organisms of nonvertebrate structure, and we must not insist that for the universe as a whole, culture is limited to bipeds (cf. Howells 1959:344-45).

The complexity of the anatomical adjustments required for easy bipedal locomotion (cf. Delmas 1958) makes it unlikely that hominid bipedalism came suddenly as the result of a few convenient mutations, or by genetic drift in a tiny, isolated population (Keith 1949:169). Nor does it help much to speak of trends (Hill 1950) without accounting for them, and without showing how even a slight change might be biologically useful *at the outset*. Simpson has dealt harshly with all such nonexplanatory suggestions (1951) and goes on to cite the development of human upright posture as a good case of specialization which broadened rather than restricted the adaptive type.

Washburn (1951:71-72) once advanced an endogenic hypothesis for the backward bending of the hominid ilium and the change in the function of gluteus maximus as responses to the need to keep the bony birth canal in suitable relation to fetal head-size. Recently (1960b:73-74) he has set the time for this obstetrical crisis after the assumption of habitual bipedalism and suggests that it was solved by the delivery of the human infant at a relatively earlier stage of fetal development.

Gradual acquisition of bipedal facility need not have been at the expense of earlier useful motor habits, at least not for a very long time. W.E.L. Clark (1954b:71-72; 1955:153) notes that the *Australopithecus talus* indicates retention of a more flexible and mobile foot than modern man's; perhaps for his size and weight, *Australopithecus* was still an expert climber. But even modern humans, when suitably trained from childhood, remain excellent climbers, and if in good condition, certainly exceed gorillas in acrobatic ability. Nor have all of us utterly lost our ability to run on all fours (Fig. 1c; cf. Hrdlička 1931). Dart (1959a) calls attention to the expertise of the Bushmen imitations of quadrupedal animal gaits in games and dances. In dealing with a creature such as man, we must not overlook the probability that all cultural systems have a certain vested interest in making the distinctions between man and other animals as sharp as possible (Dart 1959a:223; Patrides 1958). Humanity is self-consciously bipedal.

The long-argued issue of brachiation or nonbrachiation as a stage in man's evolution is not crucial here (Gregory 1928a, 1928b). Our ancestors obviously avoided the seeming dead-end represented by the long-armed apes. The Hominidae might have arisen from a generalized terrestrial pronograde "ape," or from a quasi-pongid brachiator lacking the extreme specializations of the modern anthropoid apes. The fact that humans use their hands quite naturally

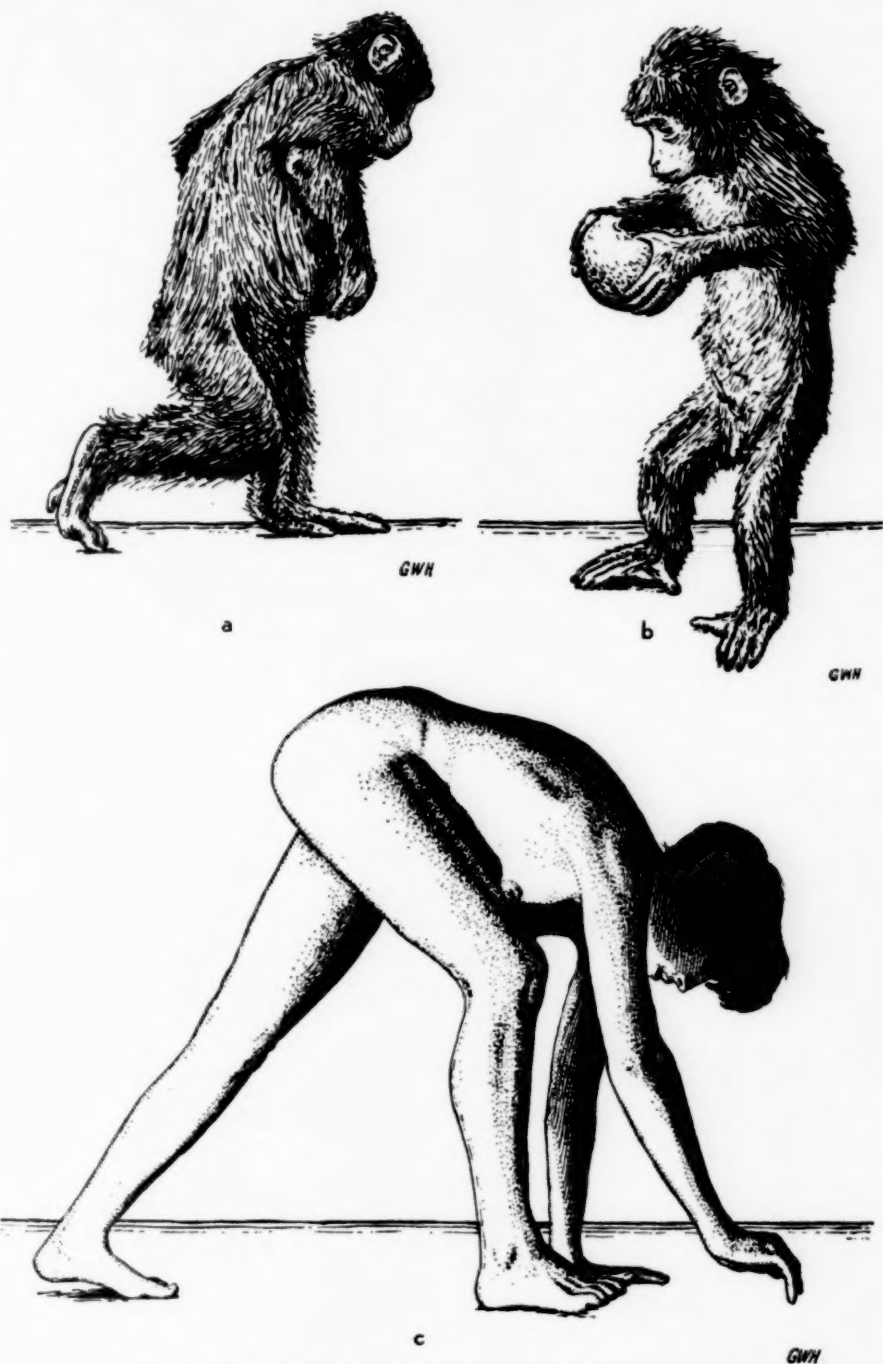


FIG. 1. Primate bipedalism and quadrupedalism (from photographs).

- a. Young stumptail macaque ♂ walking bipedally, carrying grapefruit.
- b. Young stumptail macaque ♂ standing bipedally, eating grapefruit.
- c. Adult human ♀ walking quadrupedally, showing spontaneous plantigrade use of hands.

in plantigrade fashion when walking or running on all fours is probably quite significant, as several authorities have noted (Fig. 1c). The question of the precise geological placement of the separation of the hominids from the main ancestral stock (including the ancestry of the modern Pongids) is not crucial to our argument either (Straus 1953:89), though it might become relevant in some future assessment of fossil prehominid and paleoecological evidence (Simons 1960).

For Hooton, increased body size brought man's ancestors out of the trees, as it seems to have for the gorillas (1946:134-135). Since they were "far more intelligent" than present-day apes, our ancestors quickly made the "supremely intelligent choice" of becoming bipedal, which not only increased their visual range by elevating their heads, but emancipated their hands for weapon-wielding, tool-using, and food-gathering (p. 138). This rationalistic interpretation is quite implausible and, with respect to accounting for increased body size, about as convincing as an attempt to explain the size and habits of whales by the fact that their ancestors grew too big to remain on land. Yet Hooton dismissed with justifiable wit J. R. de la H. Maret's theory that human upright posture was the end-product of skeletal responses to a diet overly rich in calcium. Another unlikely theory is that human bipedalism was a response to cold ground and snow-cover (not so absurd in the case of penguins, perhaps). Köhler remarked that his chimpanzees walked upright when the ground was wet and cold (1959:278-79). Coon (1959) agrees with me that the snow or wet-ground factor cannot have been important in the evolution of hominid bipedalism. Hardy (1960a,b,c) attributes man's upright posture to water support in the littoral aquatic environment which he most ingeniously credits for man's numerous and peculiar deviations from the standard Primate pattern. Hardy's revolutionary thesis does have the merit of focusing attention on ecological determinants, rather than on inexplicable drives to master tools and weapons.

The commonest explanation for the origin of human upright posture and gait relates these to tool and weapon using, and to offense and defense. Reynolds (1931:311) was almost alone in considering simple dodging, quite apart from tools or weapons, in defense or pursuit, as a crucial factor in the final switch-over to regular bipedalism. Bartholomew and Birdsall (1953) have given us a remarkably vivid reconstruction of the ecology of the fossil protohominids, in which they stress the importance of bipedal locomotion. Even though ordinary Primate bipedalism is inefficient and infrequent, "a significant non-locomotor advantage" must have accrued from even the partial freeing of arms and hands. Agreed. But they go on (p. 482) to say that this advantage lay in the manual use of adventitious tools and weapons, adding, "only man has his locomotion essentially unimpeded *while carrying* or using a tool" (my italics). Tool-carrying is mentioned but not stressed as a factor in hominization by Washburn (1960:69), and by Hockett (1960:96). Etkin (1954:136) discusses Primate habits of carrying infants and the implications of infant transport on the efficiency of a "hunter anthropoid," but fails to mention food transport. These, and a general statement by Mason (1896:255-56) are almost the only ones



I have encountered in a fair sample of the literature dealing with human evolution which make specific mention of the possible importance of carrying activity. My disagreement is only over the nature of the burdens carried by the incipiently bipedal protohominids and the relation of this carrying to the emergence of habitual bipedalism.

A long list of names can be cited for the notion that "freeing the hands" for tool-using, weapon-handling, food-gathering, and self-defense was the main cause of hominid bipedalism (Darwin 1871; Haeckel 1900; Carter 1953; Hill 1954; Shapiro 1956; Washburn 1959:24; Oakley 1960:322). Cause and effect are blurred in many presentations. Some authors believe that increasing demands on the hands led them to be freed from locomotor tasks; others seem to regard bipedalism as a happy accident which released the hands for manipulation. Soviet theories of anthropogenesis stress the freeing of the hands as a response to the need to manipulate tools. Kosven (1957:15) writes, for example: "This development of the hands in the process of the improvement of the entire human organism followed the development of the upright posture." Neither Engels (1876-1950 ed.) long ago, nor Kosven mentions carrying, though Engels vaguely spoke of "other functions increasingly devolving on the hands." Washburn (1960a), disagreeing with my notion of food transport, adheres to Darwin's idea that tool-using was the key to upright posture. He states, surely without possibility of disagreement, that use of a digging stick would easily double a baboon's food-supply, and that stones suitable for food-grubbing would also be advantageous, adding that such tools would have to be carried along, and that this would be nearly impossible in living monkeys and apes. I think he exaggerates the difficulty of transporting a light stick; most apes or monkeys can carry a stick tripodally for fairly long distances without fatigue or, in open country, cross-wise in the mouth, just as domesticated dogs can be readily trained to carry sticks.

I do not quarrel with the idea that the hands have played an important role in the evolution of man and culture, but I find it very difficult to believe that the use of tools or weapons was of such importance in the primordial stages of hominization that it elicited habitual bipedalism. My thesis is that the arms and hands were needed for something other than locomotion: the carrying of food, and that only a bipedal gait could permit them to fulfill this need with real efficiency. Bipedal locomotion can be elicited instantaneously in macaques (and probably most other infrahuman Primates) by presenting a sufficiently bulky food burden and subsequently threatening to take it away. Bipedalism will be maintained as long as the animal is pursued, provided he does not find a safe perch or is not so terrified by his pursuer that he abandons the food burden and reverts to the more rapid quadrupedal gait. If the pursuer halts, the animal may stand erect, attempting to stuff the food into his mouth, but warily poised for further bipedal evasion.

Spuhler refers to the freeing of the hands for manipulation and sensory inspection, etc., as a "quantum leap" (G. G. Simpson's term), possible upon the achievement of upright posture (1959). Two weighty considerations force me

to reject this view. First, monkeys and apes are constantly busy with their hands in nonlocomotor activity—grooming, grubbing food, inspecting, picking, peeling, and conveying it to their mouths; they also exhibit some rudimentary tool-using (Köhler 1959). Hyperactive manual behavior has probably been a characteristic of the Primate Order since the Eocene. Since these manipulations are biologically advantageous, and if “freed hands” are positively correlated with upright posture and bipedalism, then most of the Primates should be habitual bipeds by now, with the anatomical modifications associated with human bipedalism: broad, twisted ilia, shortened ischia, etc. Second, these manual activities do not in fact require upright standing or bipedal locomotion. Even in modern man, I would guess that 80 percent of all tool-using is accomplished in sitting, squatting, or kneeling positions (Hewes 1955; 1957), and in particular, when the tool-using has to do with cutting, pounding, scraping, or splitting of food (outside of Western and a few other cultures). Ancient hominids probably very seldom stood up as they worked with their simple tools; they certainly did not stand up to work-benches. Though stand-up tool-using exists, notably in agriculture and forestry, even the digging stick is manipulable from a squatting position—and is so used among the Australian aborigines.

Weapons, on the contrary, often demand an upright stance for effective use, unlike other kinds of tools. Darwin stressed the defensive advantages of erect posture when coupled with the use of stones or clubs, or in attack, “or otherwise to obtain food.” A man-sized animal could not deliver a very crushing blow while squatting or sitting on the ground unless his victim were quite small. Jabbing with sticks or hurling stones would be most efficient from a standing posture. Without weapons, a fighting hominid at least starts to fight standing up. Dart (1953) has graphically set forth the advantages of a pivoting trunk on a firmly balanced bipedally supported pelvis, for the supposedly club-wielding *Australopithecinae*. In a recent work (1959a:195) he states that clubs must have been decisive in getting protohominids up on their hind legs. Dart reasons that australopithecines, trekking across grasslands, though originally quadrupedal, would develop habits of standing up to search for food or spot enemies (p. 196), unstable and vulnerable as this stance may have been. As a consequence, handy sticks or bones were picked up as defense weapons. Stone-throwing and club-swinging could only have been effective from a standing position according to Dart (p. 202). Anatomical changes facilitating more upright standing and locomotion came as a result of natural selection under these conditions. For Dart, the persistent use of these weapons was “the only habit capable of shifting the weight of apes from their knuckles and buttock bones onto their feet” (p. 124). Meanwhile, a skull perched on top of the spine ceased to be useful for the aggressive use of the jaws. Evidence for a dental reduction already underway among the prehominids prior to their assumption of regular bipedalism would strengthen this reasoning. With small canines and diminished mandibles, the quadrupedal habit of “leading with the head” would cease to be advantageous.

Nevertheless, I am inclined to be skeptical of this argument. I seriously doubt that the primordial bipedal hominids were skilled hunters used to wielding clubs or spears or doing much real damage with stones. The archeological evidence suggests that effective tool using (and tool-transport) came at a later stage in human evolution. Adventitious tools and weapons were surely picked up now and then, as aids in the food quest or for defense, but this seems an inadequate means to transform an occasional and reluctant biped into an efficient, habitual one. In view of the hardships faced by modern primitive hunters, it is hard to see how the South African man-apes could have survived as genuine predators. Modern hunters have a troublesome task in spite of being equipped with carefully prepared weapons used with the skill developed from long practice, their ability to use language to coordinate the actions of a group of pursuers, their frequent use of dogs, and fire—not only for fire-drives (cf. Stewart 1956) but in night-camps while in pursuit of game for several days at a time. Even if the australopithecines were effective hunters, ill-equipped as they were for it, big game hunting can hardly have been the principal subsistence pattern of the ground-dwelling, erect-walking or erect-running Primates from which they apparently descended. A long preparatory phase is indicated, in which prehomimid bipeds might have been meat and marrow eaters, but seldom actual hunters.

Some theories of erect human posture are based on the visual advantage of being able to survey the surroundings (Dart 1959a:223); chimpanzees stand up fleetingly to scan the countryside. Oakley (1954:12; 1959a:1) stresses the value of raising the level of vision by upright posture during movements through tall grass gaps between forest corridors, as a result of which "bipedal abilities would have had selective advantages." Coon (1954:14) suggests that an emergent flesh-eating ground-dwelling ape would gain from an ability to scan the landscape for game. Howells feels that bipedalism was perhaps "encouraged by environments with high grass and only thin woods, like much of present Africa, the high grass permitting the little 'apes' to stand and walk erect" (1959:127). Leakey envisages bipedalism emerging in connection with flight from predators in high grass, which would induce a cursorial protohominid to pop up from time to time to keep his bearings on a safe tree, etc.; eventually the head would be kept aloft continuously by bipedal running (1959). Vertical or near-vertical posture certainly seems related to visual activity in some animals—e.g., prairie dogs, and something of the sort may partly explain the emergence of birds from bipedal, cursorial reptiles. It is curious, however, that the advantages of bipedalism have not led to a more widespread adoption of this habit among both predators and their prey in grassland environments. Of course, Primates are far more dependent on vision than are other mammals. The olfactorily deficient Primates might find bipedalism under such conditions more valuable than if they had noses like most other mammals. Undoubtedly, vision has played some role in hominid bipedalism. Visual data are extremely important for the spatial orientation of humans, precariously balanced on two hind feet, in contrast to quadrupeds standing foursquare with

little effort. Orione (1950) has assembled data on the visual worlds of man and other mammals. The principal weakness in this visual argument seems to lie in the sporadic nature of visual stimuli for bipedalism, in which hereditary proclivities favorable to *sustained* bipedal locomotion would not have much chance to be selectively favored. Existing monkeys and apes can stand up well enough as it is for occasional visual surveys of their environments. Bears probably stand up even more frequently, and their ancestors have presumably been doing so for several million years, but bears have not become habitual bipeds. Evidently sporadic, though admirable, bipedalism has not put enough strain on bear anatomy and physiology in all that time to elicit, through natural selection, bipedal modifications in their pelves, limb-bones, and associated musculature.

Mention of bears and bipedalism suggests the adoption of bipedal stance or gait as a part of "bluff" or threatening behavior, as in the habit of male gorillas when charging (*Life* magazine 1960; cf. Dart 1959a). A female Japanese macaque studied at the University of Colorado has been observed standing fully upright and unsupported when in an apparently threatening mood toward safely caged *Cynomolgus* monkeys. Standing up makes any animal look larger in the eyes of an observer, and thus more formidable. Lorenz and Tinbergen have elegantly analyzed analogous threat postures in birds.

Portmann (n.d.:183) has dealt with other aspects of the visual world and animal form. He notes that animals with the most complex cerebralization have heads most sharply distinguishable (p. 211). Man's upright posture dramatically emphasizes the head-pole of the body, though perhaps no more than antlers do for many deer. Piveteau (1957:315) recalls that erect posture was the hallmark of mankind for Aristotle, who believed that human thinking was somehow related to it. "Human beings are proud of their erect posture," writes Dart (1959a:223) in calling attention to the symbolic value of bipedalism. Patrides traces Renaissance ideas on man's upright posture (1958). It does seem significant that the genital "pole" (Portmann's terminology) in man is presented in the frontal plane, along with the expressive aspect of the head. The rump area, so prominent visually in Old World monkeys and apes, has been tremendously transformed by the expansion of the gluteus muscles as a function of bipedalism. Freud (1930:66) also remarked on this shift of the genitalia to the frontal plane, and the later visual emphasis provided by the pubic hair, contrasting with the relative hairlessness of the rest of the trunk. The permanently enlarged female breasts in the same frontal plane are also without parallel among other Primates. Etkin (1954:138) suggests that important socio-psychological changes followed the assumption of ventral rather than dorsal habits of copulation in the protohominid bipeds, presumably concomitant with increased cortical control of sexual behavior. These matters are certainly somehow linked to the assumption of regular bipedalism, but they appear to be consequences rather than causes. Their sexual-selective value, if any, may have been a fairly rapid dividend of bipedalism, later enhanced by the drastic thinning of body hair.

The evolution of the Pinnipedia from doglike or bearlike ancestors offers an instructive parallel to the problem of hominization (cf. Davies 1958). Seals and sea-lions are a suborder of Carnivora whose ancestors took to the water and in time became specialized swimmers and aquatic feeders. In explaining their evolution, it is unnecessary to account for the swimming abilities of the dog-bear ancestors. The Polar bear swims very well indeed! From the available evidence it seems clear that the Pinnipedia emerged as a result of: (a) life in an environment, probably around the shores of the Tertiary Arctic Basin, which provided diminishing opportunities for preying on land animals, but (b) increasing opportunities for a diet of fish and other aquatic animals, (c) on the part of carnivores with swimming abilities equivalent to those of many other terrestrial carnivores *whose descendants never developed into aquatic mammals*, (d) possessing fur and subcutaneous fat with excellent insulating qualities for terrestrial life under subarctic conditions, transferable to aquatic life in very cold waters, and, very important, (e) without effective competition in this particular ecological niche. This digression is to focus attention on the central question in the origin of hominid bipedalism: what were the external ecological conditions under which it would have paid off in terms of biological advantage to adopt a new way of life involving only the increased use of a behavioral capacity already present but previously infrequently utilized?

Spuhler comes close to my hypothesis when he says (1959:6): "The change to a partially carnivorous diet had extremely broad implications for the social organization of the early hominoids. Carnivores get a large supply of calories at each kill. This concentrated food is more easily *transported* [my italics] to a central, continually used shelter than is a low-calorie plant food, especially before containers were available. . . . Compact animal protein high in calories is a good basis for food sharing." Eiseley, too, approaches the central point in this hypothesis, when he says, after stating the difficulty in seeing precisely why only one group took to bipedalism (1953:70): "Some factor coming at just the right time in man's anatomical evolution must have provided the impetus for a new adjustment . . .," so that, with the hands divorced from the job of locomotion, the new upright ancestral hominid could "move things from place to place." Eiseley did not labor this point, but I intend to, since the origin of the bipedal habit seems to me to lie in this *moving of things from place to place*. Dart (1959a:223) mentions transport habits as differentiating human children from chimpanzees of the same age; humans "carry, drag, or manipulate" objects while upright, whereas young apes prefer to carry in the mouth or to drag with one foot. Modern human children have relatively smaller and weaker jaws than young apes, but at the Australopithecus level such differences were not so striking, although bipedalism had already developed. Köhler mentions that his chimpanzees walked upright when their hands were full (1959:278-79), and that two of them stood up to collect large bundles of green weeds and would then rush into their dens thus loaded (p. 245). Leakey (1959) observed such behavior in his pet baboon when carrying ears of maize, and we have seen such behavior repeatedly in stump-tail macaques being studied at

the University of Colorado. Dart states (1959b) that he may not have given sufficient weight to burden carrying as an operative factor in the assumption of erect posture, though he feels that weapon carrying and food transport would have been concurrent behaviors.

I believe that the only activity likely to have had the capacity to transform a mainly quadrupedal ground-dwelling Primate into an habitual biped would have been food transport from the places where food was obtained to a home base where it was consumed. Such carrying could only be accomplished efficiently by an animal with arms and hands for grasping and holding. Most mammals carry things, including their young, with their teeth and jaws; even tarsiers do this. But size and weight limits for mandibular transport are much lower than for loads borne on the back, shoulders, head, or clasped against the side or chest. Primates above the Prosimii regularly assist with hands and arms in the transport of nursing infants (Jones 1926:148). Dorsal carriage of burdens might be related to experience with carrying infants on the back, but experiments with stumptail macaques suggest that the less efficient clasping of the burden against the chest or side comes more naturally. Dart thinks infant carrying habits might have played an important role in later burden carrying, especially in open range environments, and when infant dependency had been prolonged (1959b). Etkin (1954:135-36) notes that in macaques and baboons the young are no hindrance to their mothers who carry them while foraging for plant foods. But the advent of hunting might have led to a sexual division of labor. Other mammalian predators do not hunt with their young clinging to their bodies, and protohominid females thus encumbered would have been rather ineffective hunters. Loads too heavy to carry in the jaws can of course be simply dragged along, by mouth in the case of most quadrupeds, or with one hand (or foot) in the case of Primates, thus avoiding a need to walk upright. Apart from the abrasion of the load, dragging is inefficient at distances on the order of a mile or more, except over unusual surfaces such as smooth ice or sand.

Let us assume that the burdens transported by the emergent bipedal protohominids consisted of animal carcasses or large parts thereof—whole limbs, hindquarters, heads, etc. Eiseley (1953) suggests that carrion-robbery may have been important in human evolution, and Bartholomew and Birdsell suppose that the Australopithecinae were regular scavengers (1953:490). Oakley (1959a:7) sees the protohominids using tactics of intimidation to facilitate their scavenging, prior to the shift to regular hunting of large game. J. D. Clark (1960:311-12) also supports the scavenging theory, noting that some recent primitives such as the Strandlooper Hottentot practiced it, and the Mashi River people (Bechuanaland) locate carrion by watching the flight patterns of vultures. Dart has called repeated attention to the bone-collecting proclivities of the Australopithecinae (1956a, 1958a), taking pains to show that hyenas cannot have been responsible for the selective accumulations of bone represented in the bone-breccias of the rock-shelters occupied by the man-apes (cf. Howell 1959:838). He has also tried to demonstrate the artifactual nature of what he calls "osteodontokeratic" material (but cf. Washburn

1957). The tool-making capacities of Lower Pleistocene hominids have been confirmed by the Zinjanthropus finds (Leakey 1960:24-25), but the bone-breccias must mainly represent the left-overs of carnivorous meals. The bone masses are testimony of transport, from hunting or scavenging territories probably many miles in diameter. Washburn remains doubtful about the importance of carrion scavenging in protohominid evolution, and his recent experience in African game-reserves led him to suggest that there would not have been enough carrion around to support a terrestrial Primate population (1960a). Against this one must place the fact that vultures and hyenas are not rare animals, and that they and their ancestors have found enough carrion to live on for a good many million years.

If certain pre- or protohominids came to live off carrion, why would it have been necessary for them to transport fragments of it back to their dens or lairs? Why would they not simply have consumed it on the spot? For such a diet to provide the mechanism for converting quadrupeds into bipeds, we have to assume that food transport became a necessary and regular part of the new feeding pattern. For a terrestrial Primate turned carrion-robber, there would have been an element of danger in hanging around a kill-site, to which the original predator might at any moment return for more feeding. This would be more perilous than for vultures, which, unless fully gorged, can make an airborne retreat, or for hyenas who can trot off at high speed. But the major reason why a Primate carrion-eater would tend to carry his food home would be because of the inadequacy of Primate teeth for cracking bones and dissecting tough combinations of hide, flesh, and tendon. Vultures are toothless, to be sure, but they have hard, sharp raptorial beaks and claws. To maximize the food value of meat and marrow, a Primate must work deliberately picking, gnawing, and chewing slowly. Experience with feeding raw meat and bones to our stumptail macaques indicates that it takes them eight or twelve hours really to clean a beef joint and to remove the accessible marrow. In intact bones, the marrow could only be reached by cracking or smashing the bones against rocks, other bones, or with hammerstones—behavior so far not exhibited by our macaques. A macaque handles the problem of getting meat off a bone in much the same way that a human does, making no attempt to crush it with the teeth.

The initial food-transport technique need not have been expert. It is likely that spurts of running with the load clasped against the chest, as we have seen in macaques, came before the development of an even-paced stride with a well-balanced load. Running rather than walking, for an animal not yet anatomically well adapted to upright locomotion, would be easier on account of the arrangement and function of the leg muscles (Hexton 1947). Washburn (1953:503; 1960b:67) thinks that even the Australopithecinae may have been bipedal runners rather than walkers. W. E. L. Clark (1955:169-70) feels that the ancestral Miocene and Pliocene apes were like modern cercopithecoid monkeys "scampering on the ground." Oakley (1959a:1) suggests that one of them, Proconsul, could "probably run short distances bipedally." If Proconsul locomotor capabilities resembled those of the modern stumptail macaques, this

must have been a certainty. Since bipedal walkers tend to fall forward, and bipedal monkeys suffer more from this mechanical disadvantage than do humans with their well balanced skulls and reduced jaws, moving the center of gravity backward by carrying loads dorsally—on the back or shoulders—would have enhanced the efficiency of the new gait. The “natural” ways of carrying heavy, bulky objects among bipedal Primates (observable in monkeys, apes, young humans, and in any humans where the distance is quite short) are by holding the burden against the chest, abdomen, or against the side, under one arm. More efficient modes of carrying loads dorsally, used by all human carriers who have to transport heavy burdens over long distances, with or without the aid of straps, slings, etc., seem to be learned with greater difficulty. But it is at least possible that the invention of dorsal load-carrying came rather early in the history of hominid bipedalism, and that its adoption might have helped to stabilize the bipedal habit, thus intensifying and accelerating the operation of natural selective factors called into play by this new locomotor pattern.

The effectiveness of any new technique for exploiting food resources is related to body size (Eiseley 1953). To take much advantage of the carrion of the large herbivores, including the necessity of competing with other diurnal feeders such as vultures, would probably have required a body size of from 25 to 50 kilograms. But an animal of that size would have to roam fairly widely to find enough to eat in a park-savannah landscape. In this connection we must remind ourselves that neither the South African man-apes nor *Zinjanthropus* (with his specialized environment and apparently limited flesh diet) represent really early stages of hominid bipedal evolution.

— This leads us to the environmental problem. Changes of habitat are among the prime movers of biological evolution. Contemporary paleoanthropological opinion places the transition to hominid status in tropical park-savannah lands, where narrow forest environments extend along river-courses, flanked by grassy plains (Washburn and Howell 1960:37). Adaptation to such forest margins, according to Oakley (1959a:5) led to a more varied diet, and to a changeover from plant-eating to meat-eating. There is some paleontological evidence for this environmental situation as far back as Proconsul (W. E. L. Clark 1952). Clark notes that gallery forests separated by open veld must somehow have been related to the emergence of bipedalism (1955:169–70); movement from one forest clump to the next would have, he thinks, stimulated bipedal locomotion. Indeed, he suggests a parallel to the argument advanced by Romer to account for the adoption of terrestrial life by the Amphibia, forced to move overland from one pond to another in arid environments. Piveteau (1957:648) refers to this notion of the origin of bipedalism as a paradoxical effort on the part of the Primates concerned to regain arboreal habitats separated by open grasslands. It is well known that the world's grasslands expanded enormously in Tertiary times, and that rapid radial evolution of highly efficient grass-eating ungulates was underway during the appropriate phases of Primate evolution. Such herds of herbivores were preyed upon by



efficient carnivores, notably felids. Suitably generalized apes almost certainly have dwelt in the narrow bottom-land forest prongs which penetrated these herbivore-rich grassy plains. A long list of authorities accepts this general picture (Gregory 1951:534-35; Hill 1954:100-01; Howell 1954:383; W. E. L. Clark 1955:153-4,170; Teilhard de Chardin 1956; Bartholomew and Birdsell 1953; Eiseley 1953; Oakley 1957:20-21,1959a:5; Leakey 1959). We need not dwell on the earlier speculations concerned with the supposed effects of the Himalayan orogeny on hominid emergence, but it must not be forgotten that an anthropoid mandibular fragment was found in Kansu by the Sino-Swedish Expedition, and that in spite of the more numerous finds in East and South Africa, anthropoid evolution was underway on a broad front in South and Southeast Asia (Simons 1960:184,190-91). East and South Africa may well have been the locus for the appearance of bipedal hominoids, but suitable environments and potential candidates probably existed from southern Europe and North Africa to Southeast Asia as well. A museum handbook on evolution (British Museum 1958:82) discussing Proconsul, suggests that volcano-set forest fires may have driven gallery-forest dwelling Primates out into open country "where they became exposed to different conditions of natural selection." It seems more plausible to attribute the shift to more open country to changes in climate and availability of food than to the locally dramatic effects of forest fires started by volcanoes.

An environmental stimulus to explore the grassland margins of gallery forests would have been an increase in the climatic distinctiveness of a wet and dry season. Oakley stresses the role of seasonal drought in driving a proto-hominid to experiment with meat-eating in a country like the modern East African savannah (1953:28-29,236; 1957:20-21; 1959a:6), noting that baobabs may kill small game or eat carrion during droughts. In such regions, the annual dry season diminishes the availability of much of the vegetal material on which Primates normally feed. At the same time, herds of herbivores move in toward permanent watercourses and water holes, and in times of really serious drought, actually crowd into such refuges. Here they are pursued by their regular predators, so that during the dry season, kills tend to be made conveniently close to the gallery-forest homes of our postulated generalized apes. Half-consumed carcasses of big ungulates would have thus been available, not miles out in the open, parched plains, but in the river bottoms and parklands bordering stream courses. The first Primates to become accustomed to this new kind of food would have embarked on a momentous journey (cf. Dart 1959a:197).

Apes and monkeys will kill small animals for food (Spuhler 1959). In captivity, some Primates come to relish meat, and some have been raised on a flesh diet (Yerkes and Yerkes 1929). Coon (1959) mentions use of a meat cake for feeding apes in the Philadelphia Zoo. Our stump-tail macaques in Colorado will eat meat readily, and they are willing to go to considerable time and trouble to get it off bones and out of joints. Engels believed that a changed diet, resulting from a "predatory" (i.e., meat-eating) economy, would alter the "blood" (i.e.,

physiological status) of an animal, and thus play a role in evolutionary transformation. It does seem reasonable to suppose that a drastic dietary change, maintained for a sufficiently long time, would entail physiological changes upon the basis of which natural selection would work with different pressures than before. Meat, including marrow, fat, and various internal organs, provides a rich diet for Primates, and also supplies salt. The increased energy obtainable from a meat diet, compared to the low-grade roughage usually consumed by Primates, would in itself have been significant. Oakley mentions Reichenow's 1920 study in which a craving for meat was observed among captive gorillas, explained as a consequence of change in intestinal flora and fauna (Oakley 1959a: 11-12). Such an alternation of the intestinal biota might well be brought about during long droughts.

Sudden changes in food habits are not unknown among wild monkeys. Field studies of Japanese macaques have shown how a new food, sweet potato, came to be adopted by an entire local group through rapid social learning (Imanishi 1957; Yamada 1957; Itani 1955). It is reasonable to suppose that our Primate ancestors occasionally tried out new foods. The crab-eating macaques are surely the result of a shift in food habits which has become stabilized, but probably not genetically. Haldane (1956:10) emphasizes the evolutionary importance of occasional *inventions*, new habitual actions or "kriyas" as he calls them, adapting a Sanskrit word. The European tit, *Parus major*, has recently invented a method of removing milk-bottle lids to get at the cream in bottles left on doorsteps by milkmen. This kriya has spread to urban members of the species in many parts of northwestern Europe. A Primate species ought to be able to match this performance under appropriate conditions. Scott (1958:249), referring to the role of social learning in animal evolution, suggests that species with a highly developed social organization "automatically provide a situation in which the social environment, produced in part by its own heredity, becomes one of the most important forces of natural selection. . . . All these considerations lead to the conclusion that there are types of social organization and population structure which are favorable to rapid evolutionary change. . . ." The point is that successful experiments with new foods would tend to spread quickly within socially integrated Primate groups, and that if transport of a new food resource entailed bipedal locomotion, this too might diffuse rapidly (cf. Mayr 1960:371).

It could be objected that other Primates have taken to the ground in dry parkland environments, but far from becoming cultured bipeds, they have in fact become more specialized for quadrupedalism. Hooton (1925) felt that the failure of the ancestors of the baboons to develop into bipeds was a serious defect in any theory which derives bipedalism simply from conditions of life in an open terrestrial environment, but he offered no explanation for what he termed the "unfortunate choice" of the cynomorphs. Oakley suggests that the ancestors of the baboons must have had enlarged teeth when they descended to the ground, whereas the ancestors of man may have started undergoing tooth and jaw reduction prior to abandoning arboreal life (1954). More

plausibly, Coon argues (1954:13) that there may have been an initial ecological differentiation, with the proto-baboons moving into rocky, rough terrain, whereas the protohominids adapted themselves to genuinely flat country. Baboon social organization may have also played a part; no other Primates go to such extremes in maintaining male dominance by the threat to use their huge canines and long, powerful jaws. But my notion that baboons would make inadequate bipeds was rudely shaken by the recent spectacle of a troupe of baboons exhibited by a circus in Lima, Peru. With astonishing ease, though undoubtedly the result of patient training, these baboons walked and ran about as perfect bipeds, with no trace of the awkwardness that Samuel Johnson noted in dogs trained to bipedalism. Between their turns they sat quietly on chairs, with less wriggling than in a comparable group of human children. After their act, they left the ring in the quadrupedal gait, which reassured me that their remarkable bipedalism had not been surgically induced.

Mivart had raised a similar objection years ago in asking why, if high browsing were such an advantage, other ungulates beside the giraffe had not acquired giraffe-like necks. Darwin's reply was very reasonable, and Mayr brings it up to date (1960:376). We need not be embarrassed by the coexistence of baboons and australopithecines. In any case, the latter had long been bipedal and were quite capable of moving into areas occupied by baboons. Once firmly established, a carrion eating habit would encourage a widely spaced type of territoriality, promoting the hominid propensity for expansion into a variety of environments which would have become more marked with the adoption of big game hunting (Bartholomew and Birdsall 1953:487).

Out of the habit of carrying carrion to a lair could have arisen the first regular rather than sporadic use of tools. If we assume for the protohominids an intellectual level about equal to that of modern apes, no special changes would have been needed in either hand or brain to make possible this new *kriya* or learned behavior (Jones 1926; Oakley 1954:10-11; Washburn and Avis 1958:432). Tools would come to be used regularly *at the lair* because they would lie conveniently scattered about there already, in the form of bones, horns, antlers, mandibles with teeth, etc., from previous meals, and also in the form of sharp-edged rocks if the lair happened to be a shelter in suitable bedrock. This use of tools demands no foresight (Hockett 1960:95). Even dogs readily use artifacts ordinarily present in their "homes" (blankets, drinking vessels, toys, etc.), though they are unlikely to carry such objects to distant places in expectation of their future usefulness at some indeterminate opportunity. Butchery at kill-sites or carrion-sites would have to depend for a long time not on tools purposely brought into the field, but upon objects happened upon around the kill, such as fractured pebbles or sticks, or the bones, horns, and hooves of the carcass itself (Dart 1956b). Dart rejects the idea that primordial tool-using, or even tool-making, to a pattern requires speech; he believes it could be perpetuated by observation and imitation, without verbal instruction (1959a:318). I incline to agree. Social factors cannot be excluded from the effort to explain hominization, but the principle of parsimony would

suggest that we should not exaggerate the possible social differences between the earliest protohominids and existing pongids.

Spuhler (1959) considers changes in social life which might have followed assumption of a meat diet, noting that only carnivores share their food (cf. Sahlins 1959). Sharing of natural foodstuffs should be viewed more as a matter of packaging and consumption techniques than of altruistic action or a mutual aid device for achieving social solidarity. Carnivorous mammals "share" chiefly because the carcasses of large beasts are simply too big to monopolize. Herbivores do not "share" because their food is in dispersed bits, and not in chunks. If enough of a carcass were carried home by a meat-eating Primate, the meal would tend to be unaltruistically and unequally "shared" by the family or band members present, provided they were hungry and agile enough to snatch portions away from the individual who brought it home. But compared to the individualistic food grubbing practices of vegetarian Primates, even this inadvertent sharing would confer biological advantages in the form of improved family nutrition, reduced infant mortality, etc. In time, natural selection might reduce aggressiveness and individualism during meals, though modern human behavior under conditions of food shortage indicates that there may not have been notable progress along this line.

Bartholomew and Birdsall (1953:484) suppose that protohominids lived in semi-permanent family groups, whereas Etkin prefers a wolf-pack analogy (1954:136). In any case, both because of greater size and strength, and because females would be encumbered by nurslings and pregnancy, males would tend to be the more efficient food-transporters, thus laying the foundation for the later division of food-gathering labor under conditions of actual hunting.

Language probably emerged long after bipedalism, whether or not our hypothesis of a relation between bipedalism and food-transport can be verified. DuBrul has recently elaborated an attractive hypothesis for the emergence of speech as a by-product of the erect posture (1958). Oral evolution in our Order as a whole has been affected by Primate collection and preparation of food for ingestion with the hands, and not directly with the mouth. According to DuBrul, the anatomical modifications necessary for articulate speech (and not merely vocalization) depend on changes in the relation of mouth parts to the breathing apparatus consequent upon bipedal posture and the resultant change in placement of the head, along with snout reduction. DuBrul meets the usual objectors who point to articulate speech mimicry in certain birds by showing that these birds have mouth and throat structures analogous to man's, though not derived from achievement of upright posture (p. 38). I think it likely that regular linguistic communication arose in connection with the only activity of early man sufficiently complex to require a fairly extensive vocabulary of symbols: the hunting of large ungulates collaboratively (but cf. Hockett 1960). Etkin (1954:136) feels that language of some sort would have been important for a "pack-hunter anthropoid." Dart disagrees, pointing to collaborative hunting by speechless lions, wolves, etc., where silence seems advantageous. But the genuine carnivora have a highly developed sense of smell for guidance

to their prey, extremely effective personal armament, and some can move at very high speeds. Proto-human hunters without dogs, fire, or projectile weapons can be truly successful hunters only by outwitting and wearing out their victims, especially by pursuing them in small groups. In the vicinity of the quarry, the collaborative hunting language may well have been gestural rather than vocal. Before ending these remarks on language, which were to show its possible relationship to bipedalism, let me suggest the possibility of setting up an operations research or systems analysis project on the logical properties of proto-hominid big game hunting, given a knowledge of the weapons available, behavior of various ungulates in certain kinds of terrain, etc. The results could indicate the essential characteristics of any language or symbol-system necessary for economically effective big game hunting at a pebble-tool technological level. Scott (1958:250) believes that once verbal communication had appeared in small hominid bands, partially isolated by language itself from other similarly constituted hunting groups, evolutionary change would tend to be very rapid indeed.

At the beginning of this paper I said that the problem of the origin of hominid bipedalism could be attacked in part by experiments (cf. Washburn 1951:76). A small pilot study has been started in the Department of Anthropology at the University of Colorado, on a research grant supplied by the University's Committee on Research and Creative Work, using two stump-tail monkeys. These virtually tailless, sturdily built, semi-terrestrial macaques probably approach sufficiently closely the character of the Miocene or Pliocene stock from which the habitually bipedal protohominids evolved, and for this kind of study they may be superior to anthropoid apes. We are trying to determine what kinds of external conditions most facilitate bipedal locomotion, including the carrying of food-burdens, and the effects of different terrain and ground-cover. The results so far are encouraging, though our data consist mainly of still and some motion picture records (Fig. 1a,b). We are also interested in meat-eating and bone-handling behavior. In view of the assertions in the literature that nonhuman Primates can maintain bipedal gait for only short distances, we are also interested in determining just how far a well motivated macaque can travel, once accustomed to walking upright, without signs of extreme fatigue or rebellion. Eventually, with animals raised and trained to bipedalism from childhood, it might be possible to detect changes in muscle and bone growth (cf. Bunak, *in* Delmas, 1958:33). Comparative studies of several species might reveal the existence of hereditary variations facilitating bipedalism in certain species.

## NOTE

<sup>1</sup> This is a revised version of a paper read at the 58th Annual Meeting of the American Anthropological Association, Mexico, D.F., 1959.

As indicated in the bibliography, I have received useful comments and suggestions from a number of colleagues; naturally I am responsible for any misuse I may have made of these ideas. I am particularly grateful to A. J. Kelso and H. Rodeck of the University of Colorado, and W. Wilson of Bryn Mawr, for their reading of the penultimate draft of this paper, to S. Asmus for Figure 1c, and to E. C. Templeton for indispensable assistance with the experimental macaques.

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## A Reconsideration of Malayo-Polynesian Social Organization<sup>1</sup>

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TWO problems of interest to students of Pacific cultures are: (1) the nature of proto-Malayo-Polynesian social structure and (2) the relationships between Polynesian and Melanesian social structure. I present herein new field data from the Banks and New Hebrides islands which bear on these problems.

Malayo-Polynesian societies are characterized by a wide variety of kinship systems of both lineage and generation types. In Oceania lineage systems predominate in Micronesia and Melanesia, while generation systems are characteristic of most of Polynesia. Generation systems are also found sporadically in the former areas, often in what are considered as "archaic" pockets. Murdock (1949) has suggested that the original Malayo-Polynesian speech community had a social organization of Hawaiian type, including bilocal residence, bilateral kindreds, the absence of unilineal descent groups, and a Hawaiian-generation system of kinship terminology. The presence of various kinds of lineage systems in parts of the area today are interpreted as subsequent local developments from that common base. Murdock's hypothesis rests upon the present distribution of the generation type among Malayo-Polynesian speakers and on analytically derived Hawaiian-type antecedents for some of the lineage systems found at present in Oceania.

Assuming that Murdock and others are correct in positing developmental and diachronic relationships between lineage and generation systems, an alternative hypothesis is possible. This is that the generation systems characteristic of Polynesia represent breakdowns of lineage systems of the type widely distributed throughout Micronesia and Melanesia. This view is not original. It was held by Rivers (1924:60-61), but he supplied no evidence for his position, nor did he offer an explanation for the assumed transition. Burrows (1938:145) similarly suggested, on distributional theory, that the simple Hawaiian type terminology was not old, but reflected a reduction in complexity, an "abandonment" of terms. Material which I have recently collected in Melanesia<sup>2</sup> indicates that a breakdown of Crow<sup>3</sup> into generation systems has occurred in the Banks Islands and in the Barabet area of central Pentecost in the New Hebrides. The reasons for the transition in both of these unconnected areas seem fairly clear and their nature suggests that similar factors may account for the widespread occurrence of generation systems in Polynesia. Before considering the theoretical implications of this reverse sequence, I will present a brief sketch of the Banks and New Hebrides cases.

*Banks Islands kinship*

The Banks Islands are a group of seven small islands lying just north of the New Hebrides. The cultures of the islands vary, but they fit within a larger framework of cultural unity which encompasses the northeastern New Hebrides, including the northern part of Pentecost and the community of Barabet. We are thus dealing with two similar but independent cases of culture change within a basically similar cultural context.

In the Banks case we are fortunate in having reports of four independent investigators whose field work was separated by intervals of roughly forty, thirty, and twenty years, respectively. This provides a picture, however imperfect, of culture change over a period of nearly one hundred years.

Knowledge of Banks Islands social organization rests primarily on the publications of Codrington and Rivers, both of whom obtained their fullest materials from the island of Mota. For comparative purposes, the Banks data discussed here will refer specifically to Mota. The reader should understand that, with a few important exceptions and with some minor variations, the Mota characterization and the general remarks herein apply to the entire Banks area.

In assessing the published reports on the Banks Islands, it is useful to know something of the underlying field work. Codrington collected his data between 1863 and 1887 while serving in the Melanesian Mission of the Anglican Church. Although he visited the Banks, most of his data were collected on Norfolk Island where natives from the Solomons, Banks, and New Hebrides attended mission school. He twice made systematic inquiries about social organization, the first sometime between 1863 and 1866, the second in 1866-7 on Norfolk Island when he reviewed the material ultimately published in 1891 (1891: v-vi). Considering that his data were obtained largely from isolated informants away from home and in view of the state of knowledge regarding kinship organization at that time, it is not surprising that Codrington is unclear on certain points. He reported exogamous matrilineal moieties for Mota but failed to get at the precise nature of the matrilineal "families" within the moieties. His explanation of the kinship terminology is sometimes misleading, but he clearly reported and attempted to account for a Crow system.

In 1908, Rivers passed through the Banks group on the Melanesian Mission ship, "The Southern Cross." He was ". . . enabled to make a special study of Mota, at first during a brief visit to the island itself but chiefly from natives of Mota who were working on the yacht and especially from one man John Pantutun . . ." (Barnard 1924: 1). Rivers published his Banks Islands data in 1914 and, like Codrington, he recorded a Crow system of terminology for Mota. Like Codrington, also, he did not get adequate material for a proper understanding of the moiety subdivisions. He reported that they did not influence the regulation of marriage, a belief in which he was almost certainly incorrect. Apparently all of Rivers' information on the moiety subdivisions was supplied to him by Reverend Durrad (Rivers 1914: 23). In view of the secondhand

nature of his information and the fact that most of his own Mota data were not collected in situ, so to speak, it seems quite possible that Rivers' characterization of Mota kin groups could be erroneous. Rivers himself, noting that some of his moiety subdivision names matched local district names recorded by Codrington, suggested that further investigation was necessary regarding the relation between the two modes of grouping (1914:24).

It is worth noting that in 1922 Barnard, a student of Rivers, spent nearly two months in the Banks Islands, collecting data on social organization, with Rev. R. E. Tempest as his interpreter. After Barnard's return to England, Reverend Tempest continued to send him kinship schedules from the various Banks Islands. Of interest to us is the persistence of a Crow system of terminology on Mota as late as Barnard's visit in 1922. Unfortunately, Barnard failed to get fuller data on the moiety subdivisions, apparently not realizing their importance while in the field. In his discussion of the regulation of marriage on Mota, Barnard concluded that the explanation "... is more complex than that usually given 'a man may marry any woman of the other moiety'" (Barnard 1924:19).

Summarizing the data on Mota published by Codrington and Rivers, we may say that the Banks group was characterized by avunculocal and/or patrilocal residence, exogamous matrilineal moieties each subdivided into a number of matrilineal kin groups whose precise nature and function were undetermined, Crow cousin terminology, and property rights transmitted from a man to his sister's son.

In 1958 I collected hypothetical kinship schedules and other kinship data from five Mota informants who were temporarily resident or visiting at the Melanesian Mission station in northeastern Aoba in the New Hebrides. All five informants independently responded to the hypothetical schedules with Hawaiian cousin terminology. All parallel and cross cousins were designated by the terms used for siblings. This was in striking contrast to the Crow cousin terminology recorded by Codrington and Rivers and verified by Barnard. My informants responded with additional generation-type features, including the use of the term "mother" for mother's brother's wife, and "father" for father's sister's husband, in place of the distinct terms recorded for these relatives by Codrington and Rivers. Table 1 contrasts the relevant kinship terminology in the published accounts with that recently recorded. The terminology I recorded varies from a pure Hawaiian-generation type in that, although all terms ramify out along generation lines, special terminology for mother's brother and for sister's son (male speaker only)—prominent features of the previous Crow system—remain. Apart from that the system corresponds to the Hawaiian type: all persons of the grandparent generation are referred to by a single term, as are all members of the grandchild generation; father's sister, father's brother's wife and mother's brother's wife are called by a variant of the term for mother; father's brother, father's sister's husband and mother's sister's husband are all designated by the term employed for father. All cousins are terminologically equated with siblings. In the first descending generation

TABLE 1. MOTA KINSHIP TERMINOLOGIES

	Codrington 1866-67	Rivers 1908	Lane 1958
GrPa	tupui*	tupui	tupuk
Mo, MoSi	veve	veve	ravivik
FaBrWi		veve	ravivik
FaSi	vevegae	raveve	ravivik (veve)
MoBrWi		mataima	ravivik
MoBr	marau	marau	wulus (w.s. uses name only)
FaSiHu		marau	mama
Fa, FaBr	tamai	tamai	mama
MoSiHu		tamai	mama
Sibling,   C** (same sex)	tasiu	tasiu	tasik
Sibling,   C (opposite sex)	tutu	tutu	tutuak
FaSiSo		tamai	tasik (w.s. tutuak)
FaSiDa	veve	veve	tutuak (w.s. tasik)
MoBrSo	natui	natui	tasik (w.s. tutuak)
MoBrDa	natui	natui	tutuak (w.s. tasik)
Ch, BrCh	natui	natui	natuk
SiSo	vanangoi	vanangoi	wulus
SiDa	vanangoi		(name only used)
GrCh	tupui	tupui	pupua

Terms are given for male and female speakers. Where usage differs according to sex of speaker, that for a woman speaking (w.s.) is noted separately.

\* The -i suffix in the first two lists probably denotes "of," or else is an independent suffix. The -k suffix in the last list is first person singular possessive.

\*\* ||C denotes parallel cousin, i.e., MoSiCh and FaBrCh.

all persons are "child" except for sister's children (male speaker). A man uses a special term for sister's son and addresses sister's daughter by name only.

I was unable to get adequate genealogical data from the informants at hand to check thoroughly against the hypothetical schedules. There is no question, however, that the terminological usages recorded are Mota usage and are not influenced by contact with Aobans. Northeastern Aoba is characterized by Crow terminology which Aoban informants use in response to hypothetical schedules and in discussing their own genealogies.

The genealogical data were also inadequate to display the pattern of marriage regulation, but it is clear that a prescriptive marriage system existed until quite recently, if it does not still function, and that the moiety subdivisions were vital in this connection. It is my own belief, based on informant testimony and on analogy with surrounding areas, that the Mota subdivisions were until recently exogamous matrilineal clans in the classic sense. Barnard (1924:7) arrived at the same conclusion.

*Barabet kinship*

The Barabet situation closely parallels that described for Mota. Barabet and several nearby hamlets on the west coast of Pentecost are the home of a few dozen survivors of a population which at the beginning of this century numbered in the thousands. Depopulation has resulted in the loss of many features of aboriginal culture, but despite this, and despite local peculiarities, it seems clear that culturally the Barabet region is most closely affiliated with the avunculocal-matrilineal region to the north. Northern Pentecost is characterized by exogamous matrilineal moieties and clans and a Crow system of terminology. It seems likely that this pattern of social organization formerly characterized the Barabet region as well.

At Barabet today, the matrilineal moieties found farther north are recognized, but there is virtually no vestige of clan or lineage organization. Residence is bilocal now, although traditionally it was not so, and a modified Hawaiian type terminology similar to that reported for the Banks is in use. All terms ramify out on generation lines with these exceptions: special terms designate mother's brother and sister's child (male speaker only); father's sister and mother's brother's wife are classed together and designated by a term distinct from that used for mother, mother's sister, and father's brother's wife. Barabet consanguineal terms are presented in Table 2.

This modified Hawaiian system is in use among close relatives, but among not closely related persons, and in response to hypothetical kinship schedules, informants consistently use a straight generation system. That is, mother's brother is designated by the term used for father, father's sister and mother's brother's wife are called "mother," and sister's child is classed as "child" along with all other members of the first descending generation.

Unfortunately, the Barabet area is unreported in the literature and we cannot document the shift here by citing historical reports of a previous Crow system. The question may be raised legitimately—how can we know that the Barabet situation is not the reverse of that suggested here, i.e., could Barabet have a bilocal, bilateral Hawaiian type of social organization which is develop-

TABLE 2. BARABET KINSHIP TERMINOLOGY

zimbing	GrPa and all collaterals in this genealogical generation
zimang (tita)	Fa, FaBr, FaSiHu, MoSiHu
dahing	Mo, MoSi, FaBrWi
wawa	FaSi, MoBrWi
mezang	MoBr
maring	Br, FaBrSo, FaSiSo, MoBrSo, MoSiSo—all man speaking Si, FaBrDa, FaSiDa, MoBrDa, MoSiDa—all woman speaking
sizing	all siblings, parallel cousins, and cross cousins—opposite sex of speaker
duzung	Ch, BrCh, SiCh w.s., HuBrCh, WiBrCh, HuSiCh, WiSiCh
zialang	SiCh m.s.
mimbing	GrCh and all collaterals in this genealogical generation

Terms are given for male and female speakers. Consanguineal terms only are given.

ing a matrilineal emphasis through contact with avunculocal-matrilineal neighbors to the north?

Lacking written records, we must base our judgment on internal evidence. This consistently appears to favor the interpretation offered here, that the matrilineal features at Barabet constitute remnants of a previous Crow system rather than evidence of a nascent matrilineal emphasis. Data on kinship behavior drawn from elderly informants, behavior for the most part no longer evidenced at Barabet, parallels that associated with kinship roles in the matrilineal area to the north. Father's sister played a key role in almost every life crisis. When a newborn infant defecated for the first time, the feces were kept for four days and then ceremonially disposed of by the father's sister of the infant who was paid one long mat for her services. Ten days after the birth of the infant, the parents gave a feast for all the father's sisters of the child and each of these was presented with one small mat skirt. The infant's father's mother or—in the event she was not living—father's sister gave one large male pig to the mother's brother of the child "because the child was born." When the first tooth erupted, when the child first walked, and at the first haircut, the mother of the child paid one small mat to the father's sister of the child. When a girl reached about ten years of age her ears were pierced by her father's sister who received a small mat in payment from the girl's mother. It was the father's sister who informed a girl of her impending marriage and disclosed the name of her future husband. When an unmarried girl ceremonially killed a pig in gradetaking rites, her father's sister received a small mat. The constant exchange of ceremonial services and payments between father's sister and both male and female ego are characteristic of Crow societies. We may note especially the substitution of father's sister for father's mother where the latter is not available.

Despite the complete lack of matrilineage organization at Barabet, informants say "mother's brother is just like brother" and offer this to explain why mother's brother's daughter is not marriageable and why mother's brother's widow is. The foregoing roles and attitudes all fit perfectly with a matrilineal sib organization such as exists farther north. It is difficult to explain the existence of traditional rights and duties of this type at Barabet if a matrilineal clan system had not previously occurred in the area.

#### DISCUSSION

It thus appears that in both of these examples there has been an independent shift from a social structure characterized by lineal descent and Crow terminology to one with weakly defined or amorphous kin groups and with a generation system of terminology. If we consider the histories of the two areas, certain facts stand out and appear to be related to and to predicate the changes described.

The primary one is that both areas suffered relatively rapid and drastic depopulation in the contact period, in the case of Barabet from thousands to a few dozen, and in the case of the Banks a comparable though numerically



lesser loss. Barnard reported the population of Mota in 1922 to be some 400 to 500 persons. At present the figure is about 200 persons. Depopulation was undoubtedly due to multiple causes but, in both cases, introduced disease was the crucial factor. The major decline in the Banks was earlier than at Barabet, occurring at the end of the last century, whereas that at Barabet continued well into the present century.

The Banks are an area from which there has been considerable labor recruiting and was the first center of missionary activity for this region. However, apart from missionaries, there has never been any European settlement in the Banks, and from the point of view of transport, the islands have been and still are somewhat isolated.

The Barabet region is the only place on Pentecost where there is any non-mission European settlement. There is a French plantation south of Barabet, but this area also, in terms of transport, has been relatively isolated. In neither case does it appear that the specific local facts of contact directly influenced either the population decline or the change in social structure. The plantation at Barabet developed after depopulation, as a result of the population loss rather than as a determining factor. Both areas have been relatively isolated from neighboring culture areas, particularly during the initial period of European contact and the changes in their social structures appear to be primarily matters of internal evolution.

Sudden and extensive depopulation such as occurred in these two areas would have immediate effects on a rigid matrilineal clan system. Any clans which lost heavily in membership would lose important functions, even if they did not become extinct. In an area in which much clan lore as well as actual membership was secret, children orphaned young would be unaware of their clan connections. With the dissolution of these kin groups, we would expect loss of associated kin terminology. In a situation where numbers of children were suddenly orphaned, members of the adult generation, even if not of proper relationship, would have to take over their care. Given these conditions we would expect terms for father and mother to be extended to all members of the parental generation, sibling terms to be extended to all members of ego's generation, and so on. Granting that loss in clan membership might cause a lineage system to break down into a bilateral one, the question arises, why should Crow transform to Hawaiian rather than to Eskimo, the other major bilateral type? One of the distinctive features of the Crow-Omaha type is that it *lacks* cousin terms. If the transformation is a breakdown, then we expect Hawaiian terminology rather than Eskimo, i.e., we do not expect new elaborations. It seems possible that when Dakota-Iroquois cases suffer a loss of personnel resulting in the dissolution of sibs or clans that this may cause a shift to Eskimo terminology with distinct cousin terms.

A number of writers have stressed demographic factors in relation to social structure type and have suggested that small unstable populations are usually bilateral, whereas lineage systems require a more stable and possibly denser population. Netherlands New Guinea has a wide diversity of social structures

and adjacent societies have lineage and bilateral systems. Two recent field investigators, van der Leeden and Pouwer, working in different parts of the Territory have concluded that lineage systems convert to bilateral when the population drops below a certain level (de Bruijn 1958:149-50).

In a searching examination of Hawaiian social structure on a world wide basis, Dole (1957) has enumerated various features which appear to be correlated with the type. Among other factors she also cites instability and demographic shifts. It seems reasonable that multiple causes depending upon local circumstances may account for Hawaiian type organization in different ethnographic areas. In the Banks and Barabet cases, sudden and extensive depopulation leading to the rapid dissolution of clan organization appears to be the single crucial cause. Dole also concluded, on the basis of a slightly different argument from that presented here, that Polynesian systems are recent rather than old.

Turning to other ethnographic areas, we may note that shifts from Crow systems to Hawaiian have been documented by Spoehr in the southeastern United States (1947) and suggested by Eggan in some of the eastern Pueblos (1950). Explanations of these shifts usually stress internal evolution consequent upon contact with European bilateral systems, but it is recognized that severe depopulation also characterized these areas during the period of transition and we may ask whether this factor may not have been the crucial one in the breakdown of the older systems. The distinction I am making here is that depopulation rather than contact with a bilateral system may be the key factor.

Returning to the question of ancestral Malayo-Polynesian social structure, we can imagine that in the settlement of Polynesia sudden loss of components of the population must have been an ever-present hazard incidental to the migration of small canoe loads of people over great distances through dangerous seas. Also, in new and marginal environments, natural disasters such as drought and hurricanes must have left settlers often exposed to starvation and resulting loss of personnel.

Under these circumstances, a malleable social structure such as the bilateral Hawaiian type would have provided a particularly suitable framework in which survivors could maintain themselves as cooperative social groups, whereas maintenance of a more rigid lineal system would not have provided the leeway necessary in emergencies. Firth (1957:596-97) has called attention to the plasticity of Polynesian social organization and Spoehr (1949) has argued that flexibility is a distinguishing characteristic of generation type systems.

Lineal kin group elements presently existing in some areas of Polynesia can be interpreted as vestiges of earlier systems rather than as incipient developments or results of contact with areas where lineal systems exist. The fact that these features occur more often in western than eastern Polynesia does not of itself testify to closer ties with Melanesia, but may simply reflect the greater difficulties of moving into the further seas to islands with fewer resources and initially more difficult environments.

Furthermore, such features as rank and chieftainship in Polynesia may relate to the conditions and changes suggested here. Assuming small groups and difficult circumstances, it would be efficient to have a concentration of power and authority in the person of one or a few individuals who could act as unqualified leaders, and in having the inheritance structure so constituted that there was a continuing ranked succession of heirs so that leadership would function smoothly regardless of loss of particular personnel. On the other hand, in normal times the succession in the chain of authority need not have passed through all individuals but only to those having the closest direct lines of descent from the ancestral authorities, and if the new environment proved to be a fruitful one, allowing large and rapid expansion as was the case in some of the islands, a ranking system might well have crystallized in which segments of the population developed into classes without authority or power.

The Hawaiian generation type kinship system provides a framework embodying these potentialities, whereas a lineage type structure, essentially equalitarian in cast, does not within itself carry grains of automatic leadership determination, nor does it predicate automatic inheritance of leadership.

In summary, I am suggesting that proto-Malayo-Polynesian kinship could well have been of the lineage rather than generation type. This does not preclude the possibility that an Hawaiian system may develop matrilineal emphasis for some reason and matrilineages make their appearance. Any particular case would have to be assessed according to the evidence.

If the Polynesian bilateral systems generally represent a breakdown of lineage systems, the implications for interpretation of Oceanic culture history are considerable. It is obvious that in the majority of cultural categories Polynesia and Melanesia are closely related. However, there have always been several important areas of radical difference which have acted as blocks to considering Melanesia-Polynesia as an intimately related unit. One of the most difficult of these has been that of social structure in which the two regions have seemed to deviate so radically one from the other. A second area of contrast has been that of authority structure and leadership. In view of these striking contrasts, a number of scholars concerned with reconstruction of Oceanic culture history have shied away from relating Polynesia directly to Melanesia. They have either brought the Polynesians around the north end of a Melanesian barrier or have had them infiltrate it, touching here and there and leaving a few pockets, but remaining for the most part unsullied by their contacts.

I am not at this time grappling with problems of specific prehistoric relations. I am offering three propositions: (1) that the bilateral kinship systems found generally in Polynesia may represent breakdowns of lineage systems characteristic of the areas to the west; (2) that authority structure and leadership systems may be intimately related to kinship systems; and (3) that two of the major barriers to connecting Melanesia closely with Polynesia may not be barriers at all but, instead, confirmation of relationship.

## NOTES

<sup>1</sup> This is an expanded version of a paper read at the 58th annual meetings of the American Anthropological Association in Mexico, D.F., December 1959.

<sup>2</sup> The Mota and Barabet data presented herein were collected during field work in the New Hebrides in 1957-58 under the Tri-Institutional Pacific Program (University of Hawaii, Yale University, and Bishop Museum) and supported by the Carnegie Corporation of New York.

<sup>3</sup> While it is true that there are sub-variants of the Crow type, for our purposes the definition in Murdock (1949:224) following Spier (1925) suffices here. Cross cousins are classified as follows: MoBrCh is designated by the term for Ch; FaSiDa is classed with FaSi, and FaSiSo is classed with Fa. Usage of subsequent technical terms in this paper, e.g., Hawaiian, Dakota-Iroquois, Crow-Omaha follows Murdock (1949).

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## *Social-Psychological Dimensions of Ojibwa Acculturation*<sup>1</sup>

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THE interpretation of Ojibwa acculturation presented here is an attempt to relate culture to personality through four conceptual levels: institutional patterns that characterize the reservation as a subculture within American culture, role patterns that permit us to identify specific points of conflict between the subculture and its environment, the Indian self-image, and individual response to the contemporary situation in psychological terms. I have attempted to relate factors hitherto more or less ignored in culture-personality studies of the Ojibwa—such as income, public assistance dependency, drinking, and racial prejudice—to the present social-psychological adjustment of the Indian.

The analysis I have undertaken is an attempt to obviate many of the questions concerning the apparent enigmatic survival of aboriginal personality traits in the face of the extinction of the cultural conditions believed to have produced and sustained them (see Hallowell 1955:Ch.19; Caudill 1949:425; Barnouw 1950:12; Friedl 1956:815) by treating present observable events as a point of departure. I do not believe that it is necessary to propose the dissociation of Ojibwa culture and modern personality. Not only is such an hypothesis logically untenable insofar as it precludes use of behavior as the common denominator in the definition of both culture and personality, but also it does not bear up under empirical investigation. I believe that where aboriginal personality traits survive, observable persistent aboriginal cultural factors sustain them. Where such personality traits apparently survive without corresponding cultural causation they will be found to be functions of new cultural conditions and, as such, are not the same thing as their primitive antecedents.

Such a situational emphasis does not constitute a rejection of the ethno-historical point of view. It means simply that many aboriginal culture traits are extinct and cannot cause anything in the present situation, that new traits have emerged, and that we must reckon with the fact of such discontinuity. It means that where extensive cultural change has taken place, where cultural disorganization is prevalent, as is profoundly true on the American Ojibwa reservations, the researcher is obliged to select conceptual tools with this in mind. He must tighten the focus of his investigation from integrated macro-cultural forms that persist over long periods of time to micro-cultural phenomena, often disintegrated, that change rapidly, shortening, as it were, the strings of causal connection.

The bulk of the data presented in this paper are drawn from information collected on Lac Court Oreilles during a year's residence in 1951-52, and pe-

riodic visits to other Wisconsin reservations until the autumn of 1957.<sup>2</sup> Because of the nature of the data discussed, the village described in detail is called Deerpoint, a pseudonym.

#### A CONTEMPORARY OJIBWA COMMUNITY AS A SUBCULTURE

The Ojibwa community of Deerpoint is a small village on the Lac Court Oreilles reservation in northern Wisconsin. It has a population registered on tribal rolls as 113 females and 148 males. It is located among numerous lakes which are surrounded by second growth aspen, spruce, pine, and upland hardwood. Fauna include large game such as deer and black bear, though they are kept reduced to scant numbers by over-hunting, and small animals such as fox, rabbit, mink, and game birds. Soils in the area are poor; about 65 percent is sand or peat unsuitable for ordinary agriculture, the rest is sandy loam.

The village was one of the Ojibwa communities on the reservation that had to be relocated in 1922 with the development of a Chippewa River hydroelectric reservoir. Of 39 dwellings in the village, 10 are log houses, 4 are lap-board and tarpaper structures, and 25 are frame siding buildings in various states of repair. Nine are wired for electric power. Interiors vary; the better frame homes have the usual contemporary furnishings—sofas, upholstered chairs, electric appliances, and such, while the tarpaper and log houses are mostly one- or two-room affairs with rickety furniture, wrought-iron bedsteads, and kerosene-lamp lighting. Heating methods range from automatic gas and oil stoves in a few homes to homemade, oil-drum wood-stoves in others.

Communication between Deerpoint and the outside world is relatively poor. The village has but one telephone, located in the irregularly occupied policeman's house. Only the homes with electricity have dependable radios (television began to reach the village in 1953). Highway traffic flowed through the village in 1951 at about half the rate for the nearest Indian community and a quarter of the rate for the nearest White community.<sup>3</sup> The road through the village was blacktopped in 1953, however, and this has increased traffic since. The community owned ten automobiles, four of which were reliable the year around.

Deerpoint's economy, like that of the reservation generally, is a jerry-built system comprising wage labor, large amounts of public assistance, small amounts of gathering and trapping for commercial purposes, and some business enterprise. Village food supply is supplemented by some hunting, fishing, and gathering, though all three have declined in importance during recent years, and, contrary to what is generally assumed about the Indian "living off of nature," are not of major significance to the life of the village.

Figure 1 illustrates the patchwork nature of the annual employment cycle. Cash income from the various pursuits indicated in Figure 1 is estimated in Table 1.<sup>4</sup> A more detailed picture of how a single family pieces together its cash income is indicated in Table 2, summarizing an informant's estimate of his household's income in 1952. It is not representative of most Deerpoint house-



FIG. 1. The employment cycle in Deerpoint, 1952. Figures following each type of work indicate the approximate number of persons engaged in each activity, estimated in terms of day-to-day observation.

TABLE 1. APPROXIMATE CASH INCOME TO DEERPOINT IN 1952

Source	Amount
relief, pensions, and special public assistance	\$16,968
profits and wages to village from Indian-owned resort	4,000
guiding	3,500
logging (wages and sales of village-owned timber)	3,000
blister-rust control crews	2,800
salaried position (bus driver)	1,800
beadwork	1,800
bean picking	1,500
city employment	1,000
rutabaga and potato picking	900
grocery concession	500
cherry picking	500
odd jobs at local resorts	500
gathering (sale of greens, sirup, rice)	340
trapping	250
cranberry picking (tribal enterprise)	150
<b>Total</b>	<b>\$39,508</b>

holds insofar as it contains a sizable bootlegging item, and it does not include income from such sources as Aid to Dependent Children or Old Age Pensions, which may be very important to some families. Otherwise, however, it is representative of perhaps two-thirds of the village.

This income is spent almost entirely for food, clothing, beer, and the operation of the family car. Foods are simple and cheap, with heavy emphasis on starch (e.g., macaroni, potatoes) and fatty meats (e.g., side bacon, end pork

TABLE 2. SOURCES OF INCOME FOR A DEERPOINT FAMILY OF FIVE IN 1952

Source	Amount
cherry picking	\$ 364
bootlegging	300
logging	280
beadwork	100
odd jobs for local White	50
relief (grocery "orders")	50
sale of wild rice	44
odd jobs in village	28
tips, sale of speared fish, etc.	25
<b>Total</b>	<b>\$1,241</b>



cuts), varying with the financial condition of the moment.<sup>5</sup> Clothing is of the inexpensive type—blue denim, cottons, work-clothes; occasionally more is spent for a quality coat or pair of boots. Clothes are also received through charity or church-sponsored rummage sales in which, for example, a pair of pants will sell for 50¢.

Several points are clear, I believe, in such data as these. One is that as far as economic criteria are concerned, Deerpoint is a "lower class" community. Another is that most of the earned income of the village comes from work for White employers off the reservation. Such employment is an outreach of White institutions. It is especially important to the social psychology of the community because it continually exposes the Ojibwa to the values and expectations that mediate the relationship of the reservation and its environment.

The latter point is vital and is underscored by data on geographic mobility. There is continuous movement of people leaving and returning to the reservation and there is constant spill-over and assimilation of part of the population into the White culture. Not only are the Ojibwa now living in relatively compact permanent villages, in contrast to aboriginal and contact times, but seasonal movement does not isolate the individual or the family from social contact as it formerly did; it intensifies social relations.<sup>6</sup>

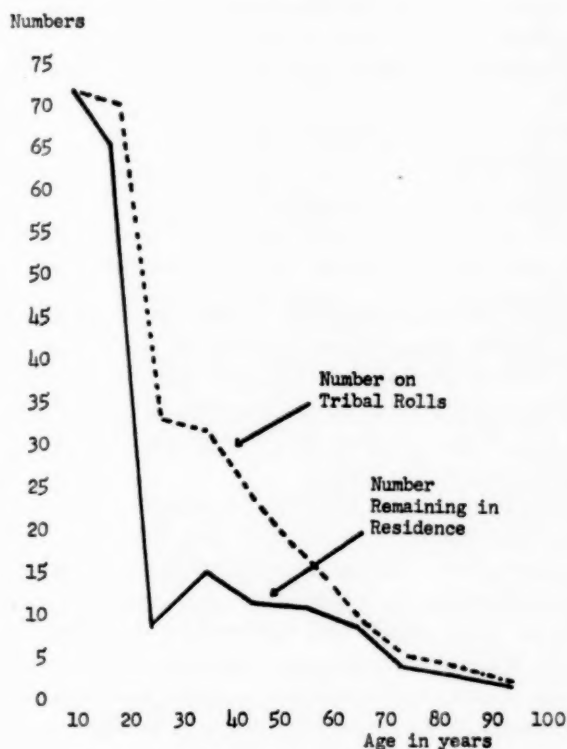


FIG. 2. Population curves for Deerpoint on March 15, 1952.

Data summarized in Figure 2 and Table 3 are evidence of the centrifugal economic and social forces affecting community life.

Analysis of the population absent from the community at this one point in time, March 15, 1952, indicates that 69 persons 18 years old or over were gone. Twelve were in old age, mental, and tuberculosis institutions. Of the remaining 57, 10 were service men, 2 were Catholic nuns, 3 were typists, 4 were semi-skilled laborers, 25 were unskilled laborers, 9 were housewives with their husbands, and the employment of 4 could not be determined. Thirty of these people had moved to Chicago, 6 to Minneapolis, 1 to Milwaukee, 8 to towns within 50 miles and others to cities and towns in Indiana, California, and Iowa.

The permanency of this relocation can be estimated. Table 3 summarizes the periods adult Ojibwa were absent from Deerpoint in March 1952. Twelve institutionalized persons (old age, tuberculosis, etc.) are not included.

TABLE 3. PERIODS ABSENT FROM DEERPOINT OF EMPLOYED ADULTS AND HOUSEWIVES ON MARCH 15, 1952

Period absent	Number of persons	Number who returned by November, 1952
Sporadic	2	0
1 week to 6 months	13	9
6 months to 1 year	2	2
1 year to 3 years	19	2
3 years to 5 years	6	0
5 years to 10 years	7	0
10 years and over	8	0
Total	57	13

The subcultural status of Deerpoint is reflected in its institutional life. In 1951 and 1952, formal organizational activity in the village was confined entirely to school and mission affairs. The grade school, a new structure with a large gymnasium, enrolled about 60 Indian children in eight elementary grades and employed two White teachers, a White janitor and an Indian bus driver. The school board was entirely White.

Both teachers lived in the village; the janitor lived off the reservation. Between them they organized a Parents and Teachers Association, league sports for the upper-grade children, and benefit dances to raise money to equip the village baseball team. Such activity always sputtered along uncertainly, however, beset by petty gossip and harassed by dissension among villagers.

The Catholic Mission's influence in Deerpoint is older and more pervasive than that of the school, but its impact is more economic than religious. Although villagers refer to the community as "all Catholic" (only four adults are reportedly unbaptized), only about 25 village adults, two-thirds women, attend Sunday services regularly.

The Mission's economic influence fluctuates with the energy and skill of the priest. Normally he lives at the main mission establishment across the reservation, but in 1949 and 1950 he lived in Deerpoint and had enormous influence on village life. He organized a woman's club, a Holy Name Society for the men, "Indian" dances for tourists, bake sales, card parties, and church dinners. He also stimulated gardening and, just prior to his transfer in 1950, had laid plans for cooperative repair and painting of village homes. When he left, most of these activities came to an end, with the exception of continued sale by the mission of small amounts of beadwork. It should be noted parenthetically that beadwork in the community was done almost exclusively for a White buyer from off the reservation. She determined both the form and price of the beadwork, usually applique strips for leather belts, and employed White women in the area of the reservation to produce identical merchandise.

Two small Indian-owned businesses operated in Deerpoint in 1952. One was a small summer concession that handled ice cream, soft drinks, bread, and some canned goods; the other was a resort establishment that rented six cottages sporadically during the summer and two to teachers during the school year. Compared to more expensive and elaborate resorts in the area, this was a modest operation and had limited economic impact on the village.

The wages paid to village lumberjacks for work in village-owned timber tracts, plus wages drawn for work in tribally-owned reservation cranberry bogs, represent the only remaining Indian business activities that affect the village.

Except for the family, the clique (defined sociometrically here as a free-choice, informal association pattern) was the only persistent indigenous group life in Deerpoint. Five to ten such groups, identified by selective association and terms like "that bunch," "him and that crowd," were discernible at any time during 1952. They often overlapped and were frequently tied into formal groups such as the PTA, informal association of teen-agers and young couples, and alliances of families leaving the reservation as itinerant labor. The clique is a medium through which a great deal of village gossip influences social relations between families.

Another dimension of community life also affects its subcultural status. There is a significant conservative-progressive distinction in household cultural orientation. Although Deerpoint has retained few obvious aboriginal traits (e.g., the last instance of totemic insult occurred around 1917; no one belongs to the Medicine Lodge, etc.), several families are said to "want to keep everything Indian." Such an attitude was sufficiently influential in village affairs to block an effort by the prestigious "progressive" Indian resorter to build tourist cottages on village tribal lands.

It is difficult, however, to select criteria that will distinguish a meaningful conservative-progressive continuum. Frequent use of the native tongue, for instance, is associated as often (in three homes) with the use of electric appliances as it is with persistence of the native food-gathering habit (three homes). Continuous use of English, on the other hand, is more frequently as-

sociated with so-called "Indian custom" marriage (in ten homes) than it is with belief in native interpretations of the supernatural (three homes.) Abandonment of a native cultural trait, in other words, has not meant its replacement with a White cultural equivalent. Loss of Ojibwa religious beliefs has not always meant Christianization, and, similarly, decline of rice-gathering or sugar-making has not necessarily meant compensatory White orientation of cash pursuits.

I am suggesting that as far as the loss of Ojibwa culture traits is concerned, the village has become deculturated and that its minimal appropriation of new cultural traits has produced a "poor White" type of subculture in which the conservative-progressive distinction actually operates as a socio-economic class line. A "poor" family tends to be considered "Indian" in cultural orientation regardless of the loss of Ojibwa cultural habits or techniques.

The point is illustrated by the so-called "Indian custom" marriage. There are sixteen such arrangements in the village. They are casual and permissive, much as was aboriginal "marriage" (see Hilger 1951:159). But it is apparent among young people who know and care little about the old tribal way of life that a liaison may be called "Indian custom" for convenience only, when financial assistance via federal revolving funds are sought, for example. But young people recognize the rationalization involved and also refer to such cohabitation as merely a "shack up."

The subcultural nature of such relations is reiterated when children are born and separation occurs. Such children are not cared for through any indigenous institution comparable to the primitive adoption system as described by Hilger (1951:33). On the contrary, they are supported via Aid to Dependent Children, a form of public assistance to Deerpoint that amounted to slightly more than \$7000 in 1952, nearly half of the public aid received by the village.

The extent to which such ADC abets the frequency of abandonment cannot be easily determined. Public welfare administrators in reservation areas insist that it is a definite contributory factor. In any case, "Indian custom" marriage can no longer be understood merely as a persistent aboriginal trait. Its meaning can only be established by placing it within the modern reservation setting.

#### *Role Conflict in Reservation Subculture*

The interaction of the Ojibwa reservation subculture and its environment can be brought into finer focus by examination of several inter-dependent role patterns in Deerpoint. Each such general role pattern can be conceived, in turn, as a cluster of specific roles.

*Economic roles.* The necessity of making a living confronts the adult male in Deerpoint with a variety of role alternatives. If he decides to conform, for whatever reason, to the expectations of local White economic values he will seek to establish a reputation as a reliable worker, eschewing at the same time the appeal of contradictory native economic pursuits such as "making rice."

The Indian school-bus driver in the village was such a person; so was a villager who worked the year-round as a resort handy man. Both were viewed by local Whites as "steady" employees; both were men of moderate habits.

Such an individual will avoid job-hopping as much as possible and will prefer steady employment at low wages to itinerant labor or guiding for higher but irregular wages. He will use his income to equip his home with modern appliances, dress his family in conformity to local White standards. Except for his racial identity he may be indistinguishable from local White laborers.

Contrary values define several distinct, but often co-dependent, subcultural economic roles. One is that of the "welfare dependent"; the other is the culturally conservative hunting-gathering role. Typically, the welfare-dependent person does not view public assistance as a temporary form of support but views it as a permanent feature of the reservation situation. He will seek such assistance ("You just go to the [county welfare office] and stand in line.") in the firm conviction that "the government should do something for the Indian." He will interpret federal relocation projects as a violation of his "Indian rights" and will support his interpretation of these "rights" with historical accounts of Indian abuse by Whites—the flooding of burials at the old village site, in violation of formal agreement for reinterment by the power company, is a ready case at hand.

The hunting-gathering role is similarly tied into attitudes toward the reservation as a refuge, a place where the Indian "at least has his rights." In fact, efforts to play this role in spite of the fact that game and wild foods have been substantially depleted in recent decades indicate very pointedly the force of such beliefs. Although rice beds in the area of Deerpoint were destroyed by the 1922 power reservoir, and it is now necessary for an individual to go through the arduous task of reconnoitering federally reserved beds 50 miles away, packing tents, boats, and equipment, and moving to the beds (often with little guarantee that the harvest will ripen as predicted), meanwhile possibly abandoning more stable wage work, "making rice" is viewed by some Indians as a precious "right" that must be exercised at all costs.<sup>7</sup> The fact that a lot of rice is sold to Whites, often to buy intoxicants at the harvest scene, and the benefits to imbalanced Indian diet thereby short-circuited, is a further complicating element.

The web of noneconomic factors shaping economic role is also illustrated in the behavior of the "guide." Guiding is an important role in the resort industry and it requires an experienced man. The notion of an "Indian" guide carries special appeal to the tourist, and resort operators are anxious to exploit these romantic connotations. The Indian, on the other hand, is equally eager to appropriate these prestigious connotations of the role. He will consequently often enlarge on his virtues as a man that knows the secrets of nature. But the point I wish to stress is that the high positive valence of the guiding role is a function of the status differentials between reservation subculture and the larger White culture. It is the psychological reward that builds up as a con-

sequence of this differential that causes an Indian mechanic in a local garage to quit his job to become a guide for a few weeks. He is not succumbing to the pull of some mysterious aboriginal personality residue.

The role conflict engendered on the reservations can be illustrated at length; I want merely to cite one further illustration. A would-be Indian entrepreneur living near Deerpoint set about building tourist cabins near the village in 1952. He was culturally "progressive" (listed on tribal rolls as  $\frac{1}{4}$  Ojibwa) and had purchased lumber, wiring, boats, and equipment on credit at White business establishments off the reservation, thus subscribing to a distinct White cultural value. When his credit would not permit him to buy sufficient materials to finish the cottages and begin operating toward retirement of his debt, the venture fell through. Pressed for payment of his debt, he used the reservation as a buffer, remarking that "Once I get [the material and equipment] on the reservation, its safe."

*Political-legal and civic roles.* The political and legal peculiarities of the Ojibwa reservation subculture illustrate the tangle of contradictory expectations that define various civic roles and, in turn, involve various economic and social behavior.

According to the terms of the Treaty of 1854 establishing Lac Court Oreilles reservation, each full-blooded Indian was to receive an 80-acre allotment to be held by descendants in proportion to their kin degree (Article Three). Such heirship tracts have since been subdivided to such an extent that it is extremely difficult to locate and get consent of heirs to use much of this land for either timber, resort, or agricultural development. Lac Court Oreilles has 221 tracts held jointly by six or more heirs (House Report No. 2503, 1952:83); single tracts may be held by as many as 50 heirs.

Lac Court Oreilles, originally 69,136 acres, had lost 14,437 acres by 1951 and the reservation was a checkerboard of Indian and White ownership. Deer can be shot out-of-season on White-owned tracts, dragged to a reservation tract to be butchered, then sold illicitly back to Whites. Muskellunge and pike can be speared by the Indians at any time on one end of Lake Chippewa while it is illegal to do so at the other end. Before the repeal of prohibition laws in 1953, White-owned taverns located on alienated tracts sold beer to the Indians in great volume and efforts by reservation authorities to keep the Ojibwa "dry" were comically ineffectual.

The fact that reservation Indians do not pay property taxes adds a further complication to the role of "public spirited citizen." Local Whites complain that they have to "carry" the Indians, pay for their schools, supply large amounts of relief, etc. These attitudes heighten White hostility and prejudice and simultaneously reinforce the Indian's resentment.

Ambiguities and jurisdictional complications of the present legal and administrative status of the reservation frustrate any effort of the Indian to take the role of "equal" or "well informed" citizen. Two individuals in Deerpoint who professed to have made strenuous efforts in the past to "do something for the Indian," one in school district affairs, the other in tribal business com-

mittee activity, retreated into cynical indifference as soon as they confronted the myriad complications of their reservation status. ("That Tribal Business Committee is just a rubber stamp.")

*Religious role.* I have already indicated that native Ojibwa religious beliefs had almost disappeared from Deerpoint by 1951. Six "conservative" households retained remnants of the old interpretation of the supernatural and from them I obtained accounts of the use of "bad medicine" and violated taboos about ten years earlier. And one man in the village occasionally participated in sacred drum ceremonies still held in a small community on the other end of the reservation. But such aboriginal religious elements survived as dislocated bits and pieces of the old way of life and were frequently ridiculed in the village, often within the same family that perpetuated them. In any case, the religious role of "pagan" has disintegrated, except where it has assumed a romantic mutant form under the influence of White commercialism.

The role of Catholic believer, on the other hand, is relatively well-defined in the village and a few individuals meet its expectations rather fully, participating in sacraments weekly, and so on. But the role is beset with disintegrating influences. Parents may, for example, actively encourage their children to become altar boys, to sing in the mission choir, or study for the Holy Orders (three girls from the village have become nuns). But the same parents may plunge weekly into drunken "powwows," during which cursing, violence, vulgarity, and sexual license occur, behavior wholly contradicting that of the "good Catholic" role.

The mission itself is sometimes victim of startling displays of areligiosity. During recent years, for example, a band of carousing villagers broke into the church and its tabernacle in search of wine. The aisles of the building were left littered with beer cans. While such sacrilegious outbursts shock the community, there is no clear evidence that they are triggered by hostile feelings toward the mission. They seem to be the result simply of the lust for drink. The areligious tone of community life is underscored by the complete neglect of the mission cemetery, in contrast to the concern shown for both aboriginal burial sites<sup>8</sup> and local White Catholic cemeteries. Deerpoint's areligiosity is additional evidence of the deculturation that is peculiar to the reservation subculture.

### *Status and Self-Image*

Although values intrude on the reservation in a pell-mell and discordant manner, they assume integrated form in several stereotypic images of the Indian. The most obvious of these is a negative stereotype of the Indian as a status inferior, a person who is considered by local Whites to be "dirty," "drunken," "lazy," and "immoral."<sup>9</sup> A counter stereotype of the romantic "Indian" can also be identified. To local Whites he is the "old time," that is, dead Indian who "never lied," "always provided for his family," and so forth; to tourists he is the "Indian" who knows the mysteries of nature, can predict weather months ahead by behavior of birds, has colorful ceremonies, and so on.

These stereotypes are crucial to Ojibwa acculturation because they are projected into the vacuum produced by the destruction of aboriginal institutions and they now dominate the reservation's accommodation to its environment.

Unlike the romantic stereotype, the negative stereotype has its roots in the vital empirical referents of existing subcultural conditions. It is the only viable system of values, in fact, mediating the relationship of Indian social life and the dominant culture. As a measure of the status differential that separates the Ojibwa from White culture it is actually an inverted definition of the behavioral change required by assimilation.

The negative stereotype is not simply a product of White imagination or bigotry. It is a White judgment, motivated by White values, concerning White experience with reservation Indians. (For that reason, too, it cannot be exorcized simply by appeals to brotherhood.) The notion that the Indian is "dirty," for example, is based upon White experience with Indian dress, housing, hygiene, disease (especially tuberculosis and venereal infection), and sexual conduct. The relative "inferiority" of the Indian to local Whites can be noted in each instance.

In Deerpoint, for instance, only about six sewing and washing machines operated in 1951; only five of twelve pumps were usable; regular bathing was exceptional. The outdoor privy was the only sanitary facility, and most were filthy and dilapidated. Two-thirds of the village's homes needed repair; all but three frame buildings had lost their paint and were badly weathered. In spite of transportation difficulties, villagers were hospitalized about twice as frequently as Whites (James 1954a:180). During the '40's, there were reportedly 20 cases of syphilis and 19 of gonorrhea in the community. During 1952, at least 90 percent of the adults had serious dental infections, many with associated rheumatic infections of the joints. During that same period the grade school was closed twice while children infected with lice were treated. I have no data in depth concerning village tuberculosis infection, but from 1931 to 1941, the tuberculosis mortality rate for Wisconsin Indians generally was eight times as high as the total U. S. population's (Ritzenthaler 1953b: 219-222).

The "drunken" element of the stereotype has parallel empirical components. I estimate that the average household monthly expenditure for beer in the village was about \$15 and that some families consume half of their cash income for beer and wine. A "powwow" of four or five adults may consume \$40 worth of beer in a single week-end. While such rates of alcoholic consumption may not be unusually high in comparison to that of some White homes in the reservation area, it inevitably works a severe hardship on many Indian families, especially when it affects care of children, diet, clothing, and the necessities of life.

As far as the stereotype is concerned, however, it is important to note that Indian drinking is highly visible. Even such humorous features of the stereotype as the claim that "all you need to do to find their towns is to drive onto the reservation and follow the beer cans" has an empirical element. It was



possible in 1952 to count from a moving vehicle over three hundred discarded beer cartons per mile (discounting those obscured by grass and undergrowth) on some stretches of reservation road. I observed nothing comparable near local White communities.

An element of hostility increased the visibility of such drinking. In 1951 and 1952 one could observe beer cartons carefully skewered to saplings along the roadside, or empty cans piled at intersections, as if to make Indian feelings toward prohibition laws abundantly clear.

Other aspects of the negative stereotype can be examined in parallel fashion. The belief that the Indian is "lazy" relates to White work ethics, to public assistance dependency, to absenteeism and apathy.

The Wisconsin Governor's Commission on Human Rights reports (1952:69) that in 1950 29.5 percent of the Indians of the state required public assistance as compared with 3.9 percent of the Whites. In Sawyer County, Deerpoint's location, the percentage of Indian population receiving public assistance was about twice that of the non-Indian population (p. 71).

Though absenteeism is frequently cited by Whites as a problem in retaining Indian workman, it is a factor difficult to study in a controlled fashion outside an industrial situation. My data are anecdotal, such comments as, "You hire them and they do good work. But just when you need them most they don't show up." However, managers at the Simpson Electric Co. of Lac du Flambeau reservation indicate that absenteeism, once a serious problem in their plant, has been reduced to levels that compare favorably with those of their Chicago plants (see also Ritzenthaler 1953c:144). This was achieved by discharge of repeated offenders as well as by the apparently stabilizing influence of employment at the factory over a long period. Foremen suggest that as the work force expanded to include about 50 percent Whites, as contrasted with an earlier higher percentage of Indians, the Indian worker became more dependable.

The decline of Indian use of reservation agricultural potential also contributes to the belief by Whites that he is "lazy." All but one of the Lac Court Oreilles's approximately 15 small farms that operated during the '20's and '30's have disappeared. Likewise, household gardens, once common (Brown 1915:45), have almost disappeared. Only two persons in Deerpoint attempted to grow anything in 1952. Failure to grow gardens was rationalized variously: "The season is too short." "There's too much quack grass." "The soil tires too quickly." "The kids steal it all anyway."

With the decline of gardening, the canning of vegetables also ceased, a valuable support to village diet was lost, and the notion that the Indian refuses to "help himself" reinforced in yet another way.

The Indian is judged "immoral" for reasons that have been discussed above. Sexual conduct is permissive by local White standards (actual White behavior is another matter) and the frequency of illegitimate births (as well as venereal infection) contribute to this component of the stereotype. Aid to Dependent Children accounts for about 40 percent of the public assistance to

Deerpoint. To local Whites, such aid connotes both "immorality" and "laziness."

Several principles concerning the development of Indian personality and contemporary behavior can be derived from an examination of the functions of the negative stereotype. It heightens the visibility of status "inferiority," simultaneously generating a desire to escape it. To assimilate, the individual must be "clean," "moderate," "ambitious," and "moral," the converse of the negative stereotype of the Indian. The degree to which the individual must approximate such standards varies, of course, with the social setting in which he attempts to "cross." But as the geographic mobility of Deerpoint's population suggested, a portion of the reservation Ojibwa continues to assimilate in just such fashion.

Movement into White culture is blocked or slowed, however, by powerful forces working against it. Economic and social dependency, real and imaginary "rights," the strong appeal of "land of our own," are several countervailing factors. But racial visibility is perhaps the most significant single barrier to assimilation. The negative connotations that words like "black" and "smokey" have on the reservations, the pecking-order attitude of the Indian toward the Negro, and the manner in which racial concerns boil to the surface during intoxication, all attest to the importance of this dimension of status inferiority. This element of the stereotype is enormously important because it anchors such "inferiority" in the biology of the individual, beyond his control. It thereby blocks adjustment to the very forces that generate the desire to escape subcultural status.

If efforts to assimilate are blocked, either by the rewards for maintaining subcultural affiliation ("rights," public assistance dependency, etc.), or by the punishment of Indian racial visibility, the individual finds himself in an impasse. To the extent that he accepts the values of White culture and defines his goals in terms of them—as inevitably he must to participate successfully in the roles that constitute the subcultural extension of White institutions, he is forced to admit the validity of the stereotype. He is pressed to internalize the negative stereotype as a self-image, to conclude that he is, in fact, an "inferior" person.

"Inferiority" does not stabilize, however, as a caste-like *modus operandi*. Such adjustment is upset by the constant enticements of White values, preachments via radio, television, newspapers, magazines, the schools, politicians, talk about being the "first Americans," "equality," and so forth. The wide range of Indian racial visibility,<sup>10</sup> moreover, prevents consistent stereotypic treatment of Indians by Whites or by other Indians. Conflict is maintained and the Ojibwa self develops characteristics of the typical "marginal personality" (see, e.g., Kerckhoff and McCormick 1955).

The tortured, devious, and ambivalent responses of the Ojibwa to the subcultural predicament are evidence that the genesis of the modern Indian self lies in the social processes that constitute reservation life, in exactly the sense that George Herbert Mead, in his classic studies, identified the origins of

"self." Contemporary personality is an isomorphic psychological version of the contemporary social situation in which it develops.

#### *Contemporary Patterns of Indian Response*

Once the outlines of the contemporary reservation situation have been drawn, I believe a good deal of the behavior of the Ojibwa becomes understandable. We can identify contemporary forms of anxiety and its causes. We can indicate, in the same way that Merton does (1938:775), the consequences of conflict between culturally induced goals and the means for achieving them. We can also indicate how the Indian seeks to conform to, get around, withdraw from, overwhelm, or escape the predicament he is in.

*Anxiety.* The anxiety that casts its shadow across the entire gamut of Ojibwa behavior is a product of both the physical deprivations that attend reservation experience (this is, of course, particularly clear for children. See Hilger 1939:39, Kinetz 1947:149; James 1954a:Ch. IV), as well as the conflicts and uncertainties that characterize status inferiority. The wholly "modern" referents of Indian concern are frequently evident; in other cases anxiety is generalized.

In Deerpoint, for example, the ancestral fear of sorcery has been replaced by new, though equally imperious, fears. Threats of an old villager in 1952 to use "bad medicine" on a neighbor were laughed off. But when a village malcontent complained to the government agent about insults of a neighbor, the agent's assurance that he would "keep an eye on the fellow" was quickly dramatized in the community to, "He's being watched. They're watching him."

These anxieties are also involved in the perpetual concern of the Ojibwa about the loss of "Indian hunting and fishing rights," "rights" that are, as I suggested earlier, of less economic than symbolic significance. One informant summed up this concern as follows.

We're losing our reservation little by little. It's getting smaller and smaller all the time. They're squeezing us more and more. Things are getting worse for the Indians. Pretty soon we'll lose everything.

Such fear that the "government is doing away with the reservations" will be expressed, nonetheless, with simultaneous resentment of the status quo: "These reservations were set up as concentration camps." Because of federal purchases of alienated lands, Lac Court Oreilles has actually increased in size in recent decades.

Ojibwa historical experience, of course, provides substantial reason for an objective Indian suspicion of "the government." "The government" has been party to outrageous fraud and violation of their "rights" (see, e.g., Senate Report, Chippewa Allotments of Lands, 50th Congress, 2nd Session, concerning a Senate investigation of logging companies; or, stipulations of the Federal Power Commission's License on Government Lands, Project No. 108, obviating need for tribal consent for power companies to flood reservation lands). But suspicion of "the government" has been generalized and has taken on some of the connotations once reserved for fear of sorcerers. Fear of "papers" and strangers illustrate.

One informant, a man with a serious heart condition, literally took his life in his hands to trudge several miles in bitter cold to my cottage in the winter of 1952 to ask if there was any hidden significance to a new income tax short-form he received for conservation work. Although he could read it and the accompanying instructions as well as I, he was troubled because "last year they were different." During the early '50's, when Senator McCarthy was powerful, a vague fear of "communists" reinforced such anxieties; tire tracks in the snow along a seldom-used tote road leading to an area off the reservation where several Polish farmers lived were interpreted by some villagers as evidence of clandestine "communist" activity, by others as indication that "the F.B.I. is keeping an eye on things."

Generalized anxiety is also reflected in the frequency of Indian emotion during drinking, as well as in the indiscriminate nature of drunken hostility. It likewise seems to produce unpredictable little breaches of the status relationship with Whites, a sudden remark by a very deferent guide, for instance, when asked by his White employer to wash out boats: "We shouldn't be your slaves. You should be our slaves!" Or a normally diffident guide comes to the door of the wife of his employer early in the morning and abruptly invites himself in for coffee with the question: "Did you make any coffee this morning?" It is such occasional "forward" behavior that causes local Whites to conclude that the Indian, like the Negro, "will take a foot if you give him an inch."

Boggs comments insightfully on such responses as follows:

Such "slips" seem to be caused by excessive anxiety. In my experience, and in others that I have heard about from fieldworkers; they seem particularly likely to happen with whites who have established a good relation with the individual. Why? I suggest because there is more anxiety involved in relations with these whites because there is something to hope for from them. Time and again with the children I knew best, demands grew with declining emotional apathy, as our relationship progressed. Often it built up to a request that I adopt them. (From personal correspondence with the author.)

*Assimilation and its barriers.* Adult Ojibwa often display clear evidence of their desire to conform to the implied demands of the stereotype. The "dirty" element of the stereotype is at work, for instance, in the following case of an individual's effort to be "White":

My informant said that when John was at the hospital he found out that Dr. D. was trying to get rid of the other doctor (a Negro) because "everybody likes him too much. . . . You know that Marie said that Dr. D. doesn't like to take care of Indians because we're too dirty? That's why we don't like him. Because he says we're too dirty to touch." (I asked, "Did he say that?") "No, but that's the way he acts. When George took Dan to the hospital they made him wait in the car for a long time because nobody wanted to carry him from the car. That's the way they treat us Indians. When Marie was there visiting someone she couldn't find any chair in the room so she sat on the edge of the bed. The nurse came in and told her to get off because she would get the bed dirty. Marie said to her, 'I'm clean. Don't worry about me. I'm clean. I changed clothes before I came here. I put on clean clothes everytime I come here. I'm clean.' . . . Isn't that awful? But that's the way they treat us. That's the way they treat us Indians."

We note that Marie defended herself by insisting that she had met White expectations. She did not insist, as she might have, that White people make too much of a fuss about cleanliness. She acknowledged the validity of the

"dirty" judgment and thereby her desire to be "White" in this respect. But like the informant, she feels that she is nevertheless inescapably a status inferior. She responds also by constructing a counter stereotype of an invariably discriminatory and antagonistic White man, at the same time that she identifies with the Negro as a fellow victim of status inferiority.

A further case illustrates how the desire to conform to the expectations of White culture may form but one feature of a complex self-image in which conflict and ambivalence are especially evident.

I had frequent contact in 1951 and 1952 with a dark-skinned, half-breed, middle-aged Ojibwa who lived with an "Indian custom" wife and family and supported his household by odd-jobs, guiding, itinerant labor, logging, and relief. Known as a very "touchy" person he seemed particularly preoccupied with the racial aspects of status inferiority. When "kidded" about his "suntanned neck" by a local White resorter he replied in cold anger, "I may be black on the outside, but I'm White on the inside and that's more than you can say."

Such intense but controlled release of hostility was characteristic of his sober behavior. A fellow logger noted that when working in the woods he "really gets mad" if a twig snaps him in the face. "He cut that little twig off and put it on a log and cut it up with his axe into tiny little pieces." During drinking bouts, he was correspondingly uncontrolled, and when the liquor prohibition law was repealed in 1953, he promptly went to a White tavern, became loud and abusive, and suffered a fractured jaw in the ensuing fight.

A field note contains a succinct version of his self-image:

I was visiting a village home and during my stay Bill came in on his way home from working on the logging operation behind the village. He had apparently gotten hold of something to drink. He began spilling out his grievances to my hosts and me. ". . . the Indian always gets blamed for everything, for the fishing not being as good as it used to be, for everything. All we get is blame, blame, blame. That's all we ever get. . . I'm Indian. I'm pure blood too. And I'm not ashamed of it. When someone asks me I tell them I'm pure blood Indian. I ain't ashamed of it. And I speak goddamned good English too. Cause I went to school and learned it. I know we're black. But there's one race that's blacker than we are and that's the black race. . . ."

We observe that Bill insists that he has met White expectations by learning good English, just as Marie insisted that she had changed clothes before going to the hospital. Like her he also implies that Whites are invariably discriminatory. He boasts of being "pure blood" (which he is not) thus making recourse to a romantic "Indian" image that will offset inferiority. He argues that the path of assimilation is blocked by his racial identity but attempts to improve his position on the racial "pecking order" by placing the Negro below the Indian.

Whether or not an individual can conform to White expectations sufficiently to "cross" depends upon his racial visibility, his work habits, and his general cultural affiliation. But under local conditions he must not look "Indian." If he is light-skinned and his dress and mien do not betray him he can assimilate locally. One family in Deerpoint had thus "crossed," even though they continued to live in the village. They affiliated psychologically with

White culture, referring to fellow villagers as "these Indians," and were considered, in turn, by Whites as "not like the rest of them." Nevertheless, they maintained high prestige in the community ("Park your car by their house. They're respected and the kids won't bother it."), which too, indicates the all-pervasive influence of White values in the village.

For most persons, however, such assimilation within the local situation is difficult, if not impossible. For dark-skinned individuals it may be necessary to leave the reservation and attempt to "cross" as a member of an urban minority. Such a course is difficult to follow. Racial visibility may force the individual to identify with minorities as unfavorably placed in the status pecking-order as the Indian. One informant, for example, on one of his few visits to the reservation after having moved to Chicago six years earlier, told me that in the city he was "treated like a nigger" and denied promotions on that account. He attempted, in turn, to avoid telling his son that they were Indian until the boy reached school age and raised questions about his race.

Such thwarted efforts to assimilate increase Indian anxiety toward life away from the reservation and enhance the appeal of the reservation as "land of our own." The consequent increased ambivalence stimulates movement back and forth from city to reservation with further intensification of economic maladjustment.

*Emotional control and indirection.* The taciturn temperament that is considered by Whites to be characteristically "Indian" is a trait that has been identified as part of the aboriginal Ojibwa method of handling emotion (Hallowell 1955:Ch. 6). I do not believe there is any doubt that distinct elements of this historic personality persist. One of the most obvious such persistencies is found in contemporary forms of hinting. A White informant married to an Ojibwa woman provided the following interpretation:

They never come right out with anything they want. You have to understand them if you want to get along with them. You got to always figure out what's on their mind. Sometimes they come around and you can't figure out what for, and if you can't tell what they're after they go away without ever telling you what they want. But they'll be mad at you. You got to learn to guess what they want from the hints they give.

Such hinting can be illustrated more specifically by the following case. It resembles closely cases of hinting described by Carver (1838:39) and Gilfillan (1901:90-91):

Crewmen of one of the village logging teams observed one of their fellows eating Spam and candy bars for lunch. They concluded, perhaps correctly in this case, that he had stolen the meat and candy from a nearby resort grocery. The meat and candy were obviously a luxury in his diet. One of the crew was instructed to insinuate that he was a thief. During one of their noon lunches the comment was made, "Well, Lou, you're eating pretty good these days. The rest of us aren't eating so good." These few hints made the point. As an informant later recounted, "He was coming to work with that meat and building a little fire and eating warm Spam while the other men had cold lunches. After Ned said that to him he stopped using it in his sandwiches, and he stopped eating candy bars after lunch too."

As far as my analysis is concerned, an essential consideration of such contemporary indirection is that it is appropriate to the subcultural situation. The individual must learn to pick his way through social uncertainties of the

moment and a hint can be an indispensable protective device. In winter, for example, Deerpoint Ojibwa are welcomed by resort taverns and groceries that operate the year around. In the spring this welcome may be abruptly withdrawn. In the words of one White operator:

No sir, they're not coming in here in the summer. I'll just put my arm across the door and ask them "Say, where do you think you're going?" I'm not going to have them in here at the bar when I have people dining here . . .

Under such circumstances a hint, a voluntary "slip," becomes a delicate probe to determine what to expect. It may be subtle enough to enable both the Indian and the White to avoid a permanent breach.

When such indirect techniques fail because the desirability of the goal is too great, the individual may have no alternative but to withdraw. Since, as one female informant put it, "No matter how hard we try, no matter what we do, our color is against us," or as "Bill" remarked above, "All we get is blame, blame, blame," it may be best to do nothing.

The goals induced by White culture and the subcultural means at hand for obtaining them at times appear almost ludicrously misfit. The account of a White teacher in Deerpoint of her disappointment with upper-grade pupils invited to participate with White children off the reservation in a panel discussion of "What Democracy Means to Me" illustrates the point:

You should have been through what I had to go through yesterday . . . You should have seen these children, the way they acted yesterday. We had a panel discussion on the meaning of democracy. It was awful. . . . Why they just had to sit down. They got up there when it was their turn and they just mumbled a little and then they had to sit down. They wouldn't even talk. I was never so mortified in my life. I told my children in here today, I told them, "If you ever, if you ever get up and act like that and I'm here, I'll see that it never happens again. I don't want you to ever act like that." And that other teacher, he said that these kids are at a disadvantage, and they weren't well enough prepared. Can you imagine that? His children get up there and make fools of themselves and that's all he has to say about it. I said to my children today, "If you ever act that way." Why they just sat up there like dummies and didn't say a thing. They had to sit down.

The fact that the topic of the discussion was totally inappropriate in light of Deerpoint's subcultural inferiority was, of course, lost on this particular White, middle-class teacher. But the incident characterizes the kind of experiences that produce Indian withdrawal.

Continuous conflict and confusion in the affairs of everyday Indian life tend to disintegrate functional forms of indirection and control of emotion. Hinting, for instance, is frequently replaced with bizarre patterns of lying. They occur between Indians as well as between Indians and Whites and are sometimes so preposterous that one can only conclude that the individual is fully aware of their probable failure. Yet they are carried through at times with outlandish aplomb. A tirade of a local White informant illustrates such lying, as well as the way in which it reinforces the White-held negative stereotype:

Honest to God, they can look you right in the eye and tell the god-damndest lies you ever thought of. You know Harry. Well, he's no damned good either. He came in here some time ago and wanted to know why he couldn't get an order (relief groceries being handled through this white).

I told him I did give him one. I said, "I took it over to your place myself." He said, "My wife says you didn't." She was with him, so I said, "Damn you! You were right with me when I carried that order into the house. What do you mean by lying like that?" She stammered around and then Harry said, "My wife left me on account of you. (They had been separated for several days), if you would have given us an order she wouldn't have tried to leave me." I walked right around the counter and went after him. I said, "God damn you! Why you get the hell out of here and don't ever come back again. . . . Why you lying sonavabitch. . . ."

*Drinking.* The most frequent cause of loss of emotional control among the Ojibwa is intoxication. During drinking bouts violence may erupt, blunt and bellicose behavior may transform a placid and diffident individual into a crucible of hostility. All the murders that occurred on Lac Court Oreilles during two decades prior to 1952 (reportedly six in number) involved use of alcohol. According to State of Wisconsin Department of Public Welfare data (1953) 14 times more Indians than Whites per thousand population come from time to time under jurisdiction of state parole and probation officers, a heavy percentage of such cases involving beatings, disorderly conduct, and threats of bodily harm during drinking.

The violence of the Ojibwa during drinking bouts is by no means a new phenomenon. Long (1791:56), Tanner (1830:164) and Jones (1860:168) each note that alcohol precipitated fighting, beatings, and killings. The violence of the contemporary drunken Ojibwa is no less dangerous and indiscriminate. Intoxicated individuals are extremely sensitive and it is a reservation social rule-of-thumb that when a drinking Ojibwa "looks for trouble" he finds it, whether or not he can find someone against whom he carries a grudge. Nevertheless, Ojibwa drinking is almost invariably a social affair. A "powwow" is, in fact, virtually an institutionalized consequence of pay day or "pension day" (when public assistance aid is received). The social function of such drinking is indicated not only by the fact that the obsessional solitary drinker is uncommon but by the fact that volume intoxicants like beer and wine are preferred to concentrated liquors such as whiskey and gin. The social nature of drunkenness on the reservation is also indicated by the actual "content" of the drinking bout. During such affairs, loud talk, shouting, boasting, and vulgar language erupt, a great deal of which concern status "inferiority":

I can get anything I want at that tavern. My name is good anywhere I go!

When (the reservation police man) caught me with that beer, I told him right to his face. "God-damn you. You got some too, you and that girl of yours." I told him right to his face too.

Extended drinking under less boisterous circumstances involves the same social airing of status grievances. The following incident is representative:

I was intercepted one day by two young couples and their children parked along a back road and given a can of beer. Friendly conversation followed and then the "touch" came, as I had anticipated. Dean wanted to sell me his bumper jack for \$1.25 (the price of a six-pack of beer) "for gas." I told him I would loan him a dollar. But without the 25¢ the dollar was worthless and thereupon he suggested I go with his wife to the nearby tavern where she could borrow a quarter. I told them I would see if I could borrow the quarter myself and left. When I returned they were profuse in their thanks and with another carton of beer we went to Dean's log cabin on the other side of the lake which he insisted I must see.

"I have my own home now." He told me he would like me to stay the night and that he



would get me a woman. . . . During the next hour Curly, Dean's companion, unburdened himself of some resentment for the white potato grower for whom he was picking recently. He claimed that he was accused of stealing potatoes and that he only took a "few small ones that they leave behind anyway." Then the two wives began to discuss their high-school education and one of them, a woman of about thirty who is almost blind, told of an argument she had with a neighbor over her training in braille. She said she felt the neighbor was trying to "undermine" her by ridiculing her education. . . . When the beer was gone Dean offered to sell me "a nice carpenter's level" (worth perhaps \$5) for \$1.25. I said I was broke. Then he asked me to drive them thirty-five miles to where their potato growing employer lived so that they might get an advance on their potato picking check. I said it was too far and that I had to go to a meeting at the nearby Indian village. Curly claimed "It's all over anyway. It started at twelve," which I knew Curly knew was untrue. . . . They were skeptical of my excuses and I left them plunged in gloom.

Certainly many of the factors I have been discussing are involved here. There is the desperate maneuver to get beer, the indirection in the story about getting "gas," seeing Dean's home, the hour of the meeting. There is the anxiety about being accused of stealing, of being "undermined" for being blind and having limited education.

My data are not sufficiently extensive or precise to prove the point, but the way in which alcohol acts to reduce the sense of isolation and to permit the ventilation of anxieties, would seem to illustrate the connection noted by Horton between excessive drinking and culturally induced anxieties (1945:161). The striking fact, too, that some Ojibwa seem to get drunk on but a single bottle of beer, when they are neither physically run down nor empty-stomached, suggests that a definite set of expectations is involved in the reservation subculture's definition of a drinking situation. Such pseudo-intoxication<sup>11</sup> helps explain the appearance of truculence and boasting before technical intoxication seems reasonable. It would also explain why many individuals are capable of staying "drunk" for several days on a very limited supply of alcohol. The reinforcement that such pseudo-intoxication provides both for the negative stereotype and the "drunken" element of the "inferior Indian" self-image is further evidence of the interdependence of social and psychological forces on Ojibwa reservations.

*The Romantic Response.* The Ojibwa's use of a romantic Indian stereotype to counter the negative stereotype of his inferiority is a sufficiently important Indian response to contemporary reservation life to deserve special comment. The romantic counterimage is maintained only because it is shared by both Indians and Whites, because it functions as a dimension of the status differential between the reservation and its environment. This accounts for much of the appeal of such occupations as guiding, dancing for tourists, and bead work. Thus, a woman that manufactures bead strips for White manufacturers will insist that "Those are Indian designs and we think them up and we ought to get credit for them"—"credit," that is, via White cultural rewards. Authentic Ojibwa bead art (see, for example, Densmore, 1913:plate 32) has long since been replaced by commercial work whose design is dictated by drugstore canons of taste. The fact that most Wisconsin Ojibwa have no better idea of what native arts constitute than do local Whites is poignantly reflected in a field note:

When we went back to my cabin we met Laura (a woman about forty years old) and I mentioned that I wanted to get someone to sing native music for recording. She said she didn't know who would be best. Then her daughter came out and said that she and several girls from town sang. And when Laura said that I was looking for Indian songs, the daughter said that they sang "The Indian Love Call," which prompted Laura to add that "They don't sing it in Indian though."

The peculiar alliance of conflicting White and Indian motives in maintaining the romantic stereotype are important enough to deserve a more detailed illustration.

A wedding was scheduled for the village mission church in which a daughter of a white Catholic welfare worker from Minneapolis was to be married. The church was chosen because of the friendship between the mission priest and the welfare worker and because, said some villagers, the priest thought such a wedding might be useful moral instruction to the village. Others said that the idea of a wedding in a quaint little Indian mission church appealed to the bride's family.

The wedding party did not drive directly to the village but spent the night before the ceremony in a nearby white community on the main highway. When they arrived the next morning they drove in with three new cars, a Buick, a Packard, and a Ford. They remained in their cars for quite a long time and villagers who were standing around the steps of the church waiting for the service stared at them unabashed. Then the bride and bridesmaid got out and posed for pictures with the little mission church as a background.

In the meantime the impatient priest sent a car back to find out what happened to a fourth auto that was to bring others of the party into the village. They all finally got to the village but one car had bogged down in the mud on the reservation road and several of the men arrived with mud to the ankles. When the service began the church was packed with curious, neck-craning villagers. A roll of white paper was unrolled down the aisle but kept rolling up. Tacks were found for the front and two little children were selected from the whispering cluster in the rear of the church to sit on the rear corners of the paper. Then, after some fretting whispers within the party about the seating order, an usher took them to the front pews. As the bride and groom moved slowly to their altar kneelers a young fellow near me grinned to a companion and said, "See, this is what you should do first, not just shack up."

After the marriage the wedding party embarked for the village hall where the bride was to be "taken into the tribe." As they filed quietly out several village boys fell to wrestling in the sand in front of the church and stirred up a cloud of dust. Another prankster set off a smoke flare behind the church and an orange cloud billowed up.

The hall gradually filled around the sides of the dance floor with interested villagers, a table was pushed into the center of the room and several attractively wrapped gifts were placed on it. Then one of the "pagan" men (a village characterization) of the community (he had not attended the wedding Mass) made a speech in Ojibwa which another villager translated (both could speak English well). The speech said that the village was grateful for the charity which the welfare worker had given it, and that henceforth the bride would be known by the name for "Ray of Sunshine." Then a beaded headband with metal clips was put around her forehead and she was given a billfold with a beaded deer design on it. Then "Ray of Sunshine" walked slowly around the room, smiling and shaking hands with villagers, addressing some as "my little friends."

The values reflected in the remark about "just shacking up" and those which motivated the wedding party are literally worlds apart. The squalor of the village, the muddy roads, the children wrestling in the dust, are in direct conflict with the fine cars and clothes of the wedding party, its meticulous concern for seating order, its definition of the situation in terms of the "little mission," "initiation into the tribe," becoming "Ray of Sunshine." The fact, however, that villagers sought to bridge the status distance between the world of dependent "inferiors" and the dominant world outside it by eagerly sharing in the romantic definition of the situation, by providing the "pagan" speech and beaded headband, all that they could really hold in common with the party from Minneapolis, underscores their subcultural predicament.

## BROADER IMPLICATIONS OF THE ANALYSIS

Space limitations do not permit me to explore many of the broader theoretical implications of the analysis presented, but I believe the approach outlined points to resolution of several outstanding difficulties. By treating personality as an isomorphic version of social process, after the manner of George Mead, it is possible to describe Ojibwa acculturation through consecutive conceptual levels: self, role, and institution, without positing any disjunction between culture and personality. A reservation acculturation model can thus be defined. Its lineaments would include: (1) the co-dependent nature of sub-cultural and White institutions, (2) the relative integration of component role systems in each, (3) the nature of values that operate between the reservations and their environment, and (4) the formation of self as a miniaturized version of the processes that characterize value conflict and accommodation on the reservations.

Such a construct permits us to examine the behavioral significance of projective data that otherwise have but limited inferential validity. Thus it is possible to identify behavioral evidence of the "psychological impasse" and "regressive version" of aboriginal personality that Hallowell's Rorschach data indicate exist on Lac du Flambeau (1955:364, 366). Kerckhoff's TAT analysis of the "marginal personality" among children in which conflict between racial visibility and White cultural affiliation is pronounced (1955) can be corroborated. Wautrous' suggestion that Rorschach data indicate a "distrust of people" in Lac du Flambeau children (1949:133) and "abrupt withdrawal" at adolescence (1949:223) can be verified by observation of behavior in the sub-cultural setting.

The subculture personality dynamics I have outlined also appear to be consistent with Gillin's suggestion that "status anxiety" functions as an acquired drive among contemporary Lac du Flambeau Ojibwa (1942), as well as with Boggs' interactional analysis of Lac du Flambeau families indicating a decline in the consistency of parent-child interaction as acculturation proceeds under local reservation conditions (1956). Both findings are corroborated by my evidence of role and value conflict on the reservations.

A reservation acculturation model could be used to make cross-reservation interpretations of Indian behavior. The Menomini "transitionals" that Spindler describes (1955:91), virtually the total Menomini reservation population, seem identical sociologically to the masses of Lac Court Oreilles. The modern Forest County Potawatomi are also very similar to both (Ritzenthaler 1953a). As for reservations outside Wisconsin, one can readily identify in the literature such common dimensions of subcultural life as status inferiority (among the Oklahoma Indians, see Howard 1955:218), economic dependency (among the Southern Ute, see Stewart 1952; among the Cherokee, see Gilbert 1943:371), or hostility toward Whites during drinking (among the Northwest Coast Indians, see Lemert 1954:356).

A cross-reservation application of the analysis suggested has significant implications for such phenomena as "pan-Indianism." Syncretism and sur-

vival of various tribal artistic remnants, for example, appears to be the consequence of common subcultural responses to status inferiority rather than the result of any natural affinity of persistent native traits. The Indian appropriates romantic "Indian" roles, in dance, music, or bead-work, because such roles counter stereotypic inferiority, because, that is, they are sustained by romantic values shared by Indians and Whites. The key to pan-Indianism, in other words, appears to lie in social relations between Indians and Whites rather than in relations between tribes.

Policy implications for the conduct of American reservations flow from the interpretation of the Indian subculture I have advanced. I cannot explore these here. But if it is correct to conclude that native cultures have been replaced by reservation subcultures of a "poor-White" type, and an essential functional requirement for their existence, as we know them, is an extreme socio-economic status differential, prescriptions that attempt to perpetuate this "Indian way of life" may be both unwise and inhumane.

## NOTES

<sup>1</sup> For convenience in discussing related literature, the Chippewa Indians of Wisconsin are referred to in this study as Ojibwa, the broader designation frequently used for people of this cultural type. I wish to thank Stephen T. Boggs for his critical correspondence and very useful suggestions in the development of my paper and the longer unpublished study from which it was drawn. I wish also to express thanks to Gordon MacGregor and Jules Henry for critical comments on early versions of the paper.

<sup>2</sup> Work with the University of Wisconsin Extension system took me into the communities and schools of Bad River, Lac du Flambeau, and Mole Lake Ojibwa reservations during 1954-57.

<sup>3</sup> Wisconsin Highway Department traffic figures indicate that in 1953, highways passing through the Lac Court Oreilles communities of New Post, Reserve, and White Fish carried some 90, 170, and 290 automobiles per 24 hours. Though White communities on Highway 70 have roads carrying twice that number of automobiles each day, Indian villages astride similar surfaced roads carry an equal number of automobiles.

<sup>4</sup> These data were collected via informants in the community. Information on public assistance was gathered in detail within the village and its gross figures checked with public assistance agencies.

<sup>5</sup> A 1942 report indicates that the people of Lac Court Oreilles got about a tenth the supply of milk and a sixth the amount of leafy vegetables estimated by health authorities as necessary for good health. (Tribal Business Committee files, 1942.)

<sup>6</sup> Judging from planimetric maps, the Lac Court Oreilles communities of New Post and Reserve have about one house per 2½ acres and one house per 3½ acres, respectively. This is as compact an arrangement of households as found in White communities in the areas; e.g., Radisson. Indian homes are scattered along the highway and in the woods in several areas of the reservation, but most of the population lives in relatively compact villages.

<sup>7</sup> During 1952, of a band of nine individuals that I observed closely from two communities, only two harvested any significant amounts of wild rice. Each of these two harvesters sold about half of their rice for about \$45 each. The poor showing of the group was blamed on drinking on the job, winds, and blackbirds. Drinking was especially demoralizing to the gathering effort.

<sup>8</sup> Armstrong notes that considerable "respect" was shown to burial sites by the Ojibwa of northern Wisconsin. The meaning of such behavior would, of course, require careful evaluation in relationship to broader aboriginal institutional patterns (1892:178).

<sup>9</sup> In Forest County, we may note in passing, the same judgments are directed at the Potawatomi. "... anti-Indian feeling in white communities is centered about three complaints: they

drink too much, they won't work or are unreliable workers, and they are unclean and likely to be diseased" (Ritzenthaler 1953).

<sup>10</sup> Tribal rolls are sometimes used by researchers to estimate degree of racial mixture on the reservations. They are virtually worthless for such purposes, insofar as such rolls as the 1952 Lac Court Oreilles roll involve considerable guesswork and no individuals are listed as less than "1/8" Indian.

<sup>11</sup> The limitations of beer as an intoxicant have been pointed out by the physiological studies of L. A. Greenburg which note the difficulty the beer drinker has in achieving and maintaining sufficient blood alcohol to sustain intoxication (1955).

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# Avunculocality and Incest: The Development of Unilateral Cross-Cousin Marriage and Crow-Omaha Kinship Systems

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## I. INTRODUCTION

IN THIS paper<sup>1</sup> we present a theory of the development of Crow-Omaha kinship systems<sup>2</sup> and unilateral cross-cousin marriage. This theory builds upon the important contribution recently made by Lane and Lane (1959:254-65) concerning Crow-Omaha kinship systems and departs radically from the theory of unilateral cross-cousin marriage presented by Homans and Schneider (1955). In this presentation we presuppose much of the analysis of kinship organization given by Murdock in his *Social Structure* (1949). Throughout this paper we shall test theoretical statements against the massive amount of data presented in Murdock's *World Ethnographic Sample* (1957:664-87).<sup>3</sup>

It has long been known (Lowie 1917:151-52) that Crow-Omaha kinship systems are associated with matrilineal and patrilineal descent respectively. Homans and Schneider (1955:34-35) have shown, and Murdock (1957:687) has confirmed, that patrilateral cross-cousin marriage (PCCM) and matrilateral cross-cousin marriage (MCCM)<sup>4</sup> are associated with matrilineal and patrilineal descent groups respectively. Recently, Lane and Lane (1959:257-58) have noted that marriage rules affect the alignment of kin types in kin groups and, more importantly, have claimed that MCCM produces the correct alignment for Crow-Omaha terminologies in matrilineal and patrilineal societies respectively.<sup>5</sup> Since they accept the view, strongly confirmed by Murdock (1949:113-83), that common kin-group membership is a major factor in common kinship classification, they are led to suggest that: "Crow-Omaha systems of terminology may be associated not only with matrilineal and patrilineal sibs respectively, but also with matrilateral cross-cousin marriage" (1959:258).

## II. SOME IMPORTANT FACTS

Before proceeding further, we must investigate more thoroughly some implications of the above hypotheses and discuss some facts relevant to them. The Lanes make two important suggestions. First, they assert that MCCM produces the correct alignment of kin types for Crow-Omaha kinship systems (since in a matrilineal society MBd, Bd, and d will be in the same lineage for a male ego, while in a patrilineal society FSs, Ss, and s will be in the same lineage for a female ego) and therefore suggest that MCCM is a major determinant of such kinship systems. Let us call this their *general* hypothesis. It

is clear at once that this general hypothesis is only partly correct. The Lanes assert (1959:257) that in a matrilineal society with MCCM a male ego's children and mother's brother's children are always members of the same kin group. But this assumes either that "mother's brother" is a single individual or else that all of the members of the indefinitely large *class* of mother's brothers marry into the same lineage. If these further assumptions are not made, any male ego's mother might have several brothers who married into several different lineages. Therefore, a male ego would have mother's brother's daughters in a number of different lineages and even with MCCM his daughter would only necessarily be in the same lineage as *one* of these. The Lanes assert (1959:258) that in a patrilineal society with MCCM father's sister's children are always in the same lineage as sister's children. The criticism here is analogous. In such societies MCCM only produces the necessary alignment if each ego's father has only one sister or if all the father's sisters marry into the same lineage. We therefore see that it is not MCCM alone which produces the correct alignments for Crow-Omaha kinship systems but MCCM combined with a rule that in matrilineal societies brothers marry into the same lineage, and in patrilineal societies that sisters marry into the same lineage. In short, it is not preferential or prescriptive MCCM alone which produces the alignments but a situation approximating or equaling *connubium*.

The second major objection to the *general* hypothesis that MCCM produces Crow-Omaha terminologies is the relative infrequency of any form of first cross-cousin marriage in societies with Crow-Omaha kinship systems. This low association has often been noted (Lounsbury 1956:176; Eggan 1956:402), and the Lanes themselves consider this objection (1959:258-59), but confine their argument to producing a few examples pro and con. No systematic test is presented. On the basis of their general hypothesis the Lanes suggest that Crow-Omaha kinship systems may be *associated* with MCCM. Let us call this their special hypothesis. We present the data from the *World Ethnographic Sample* bearing on the Lanes' *special* hypothesis in Table 1.

Clearly this hypothesis is decisively disproved. Of the 32 Crow societies for which there are full data, only 12 allow any form of first cross-cousin marriage. Of these, only 10 allow any marriage with MBd. Of the 42 Omaha societies for which there are the necessary data, only 10 allow any form of

TABLE 1. CROSS-COUSIN MARRIAGE IN CROW-OMAHA SOCIETIES

	Crow Societies	Omaha Societies	Totals
MCCM	2	5	7
PCCM	5	1	6
BCCM	5	4	9
No Cross-Cousin Marriage	20	32	52
Totals	32	42	74



first cross-cousin marriage. Of these only 9 allow any marriage with MBd. Thus out of a total of 74 Crow-Omaha societies, only 19 allow any marriage with MBd at all. Of these only 7 prefer or prescribe it. Since Lane and Lane agree that only a rule which affects a majority of marriages can significantly affect kinship terminology, it appears that the evidence is 67 to 7 against their special hypothesis.

Homans and Schneider take as given the fact that certain societies have unilateral cross-cousin marriage. They then ask what determines whether a society will have MCCM or PCCM. The association of MCCM and PCCM with patrilineal and matrilineal kin groups is explained in terms of jural authority and sentimental relations. Homans and Schneider say that MCCM arises in patrilineal societies because in such societies MB does not stand in a position of authority with respect to male ego, but rather has a relationship with him characterized by the giving of help and advice, considerable freedom, and the absence of jural authority. In short, it is a friendly relationship. Homans and Schneider see such a relationship producing marriage with MBd rather than with FSd because in a patrilineal society FS is associated with the authoritarian status of the father and the relationship with her is not intimate. They see PCCM arising in matrilineal societies for analogous reasons. In such societies, it is the MB who is likely to have jural authority over ego, while the father is apt to stand in a friendly relation with his son.

The jural authority explanation is attractive, but, unfortunately, it has several weaknesses. MCCM appears in only 34 of the 247 patrilineal societies in the *World Ethnographic Sample*. Homans and Schneider, however, explain this rare marriage rule in terms of a very general feature of the social structure of patrilineal societies. Similarly, PCCM is very rare in matrilineal societies, but receives an equally general explanation from Homans and Schneider. It appears, as has been noted by Murdock (1957:687), that these authors fail to account for the rarity of unilateral cross-cousin marriage in unilineal societies. Furthermore, they seem to ignore societies with bilateral descent. There are 204 such societies in the *World Ethnographic Sample* and at best only 4 of these<sup>6</sup> have any unilateral cross-cousin marriage.<sup>7</sup> In many bilateral societies, however, jural authority will be in the hands of the father, and MB will stand in a friendly relation to ego. How is it then that MCCM is almost nonexistent in bilateral societies? It appears to us that the jural authority theory cannot account for this absence.

Further problems are raised by Homans and Schneider's theory of unilateral cross-cousin marriage. As Needham (1956:108; 1959:217) points out, there are no known cases of *prescriptive* PCCM, while *prescriptive* MCCM does occur. Homans and Schneider's symmetrical theory of the origin of both kinds of unilateral cross-cousin marriage offers no obvious explanation for this striking factual asymmetry. Finally, Homans and Schneider do not really offer any explanation for the existence of *prescriptive* MCCM. They merely make plausible the development of *preferential* MCCM. There are thus many questions about unilateral cross-cousin marriage left unanswered by Homans

and Schneider. Therefore, although their theory is cogently argued and extremely convincing, we feel that it is not unreasonable to attempt to formulate an alternative, and it will be doubly interesting if such an alternative can incorporate an account of Crow-Omaha kinship systems.

### III. AN ALTERNATIVE THEORY

The rest of this paper will be devoted to describing in detail such an alternative theory and to presenting the factual data which we believe support this alternative. Before proceeding, however, it may be helpful to the reader to indicate here in outline what is presented in detail below.

We believe that a matrilineal society with BCCM subjected to certain kinds of pressures tends to develop MCCM and, in accordance with a *modified* version of the Lanes' general hypothesis, Crow terminology. Thereafter, however, we believe that such a society will pass through a normal progression from MCCM to BCCM to PCCM.

We believe that a patrilineal society with BCCM subjected to similar kinds of pressures passes through a quite different sequence. Here the conditions lead to MCCM and Omaha terminology, but do not result in a progression from MCCM to BCCM to PCCM.

These two sequences and the differences between them will, we believe, explain a great deal about the distribution of Crow-Omaha kinship systems and unilateral cross-cousin marriage. The theory presented below makes many simplifying assumptions and no doubt leaves many variables out of account. We believe that the closeness of fit to the data achieved by this theory shows, however, that those variables which are discussed are the crucial ones in the domain under consideration.

### IV. THE DRIFT TO MCCM

In this section we shall present in detail a theory of the development of MCCM.<sup>8</sup> Let us begin by picturing a matrilineal, matrilocal society practicing BCCM and having symmetrical cousin terms. In such a society, the core of the residence group consists of matrilineally related females; residing with them are their husbands and their unmarried sons. The husbands are, in approximately equal numbers, MBs and FSs, real or classificatory, of the women of the core group. The classificatory MBs and FSs are, of course, generally second and more distant cousins. In a unilineal society, a rule of preferential or prescriptive first cross-cousin marriage is generalized to link kin groups rather than individuals or families. Preference will be for first cross-cousins since, although one may have close relations with all members of a particular lineage, one will usually have closest relations with those relatives who are genealogically closest. In the matrilineal society we are considering that these male cross-cousins are, of course, drawn from a number of other matrilineages. Consequently, the adult male members of the residence group are not in general members of a single lineage.

Suppose that in such a matrilineal society pressures develop which in-

crease the need for cooperation among the males of a residence group, while other factors continue to require that the residential unity of the lineage be maintained. Cooperation here is meant in the widest sense: economic, military, proprietary, etc. In such a situation there are two possible types of social change. The first is a shift toward avunculocality. The second involves continued matrilocality combined with a shift in marriage practices. It is the latter possibility which concerns us here. This involves two processes which can be expected to occur more or less simultaneously.

First, a daughter will be more likely to be influenced to choose, or have chosen for her, a husband who is a real or classificatory FSs. (This is, of course, MCCM looked at from the female point of view.) The reasons for this are fairly obvious. A FSs type is a member of her father's matrilineage and is a man to whom her father stands in a close relationship as real or classificatory MB. Her father and FSs are very likely, therefore, to have structured relations with one another and to be accustomed to cooperation. Since we have postulated a situation in which male cooperation is very important, to the extent that a girl's father influences her decision as to a spouse, it is probable that the girl's decision will be in favor of a FSs type. Moreover, to the extent that other men than her father who are married to women of her lineage are also members of her father's lineage, the pressures on a girl to marry FSs will be commensurately greater.

We have considered only the motivations of the girl's parents in influencing marriage choice. Since we are considering a matrilocal society, in which a son always leaves his parents' home on marriage, it is to be expected that his parents' concern with their son's marriage choice would be practically uninfluenced by considerations of residence. The boy's parents' concern over marriage choice may, of course, be influenced by many other factors. We do not consider any of these. This stress on the girl's parents is mentioned by Burling (1958:748): "Among the Garo, initiative in arranging marriages comes from the girl or her relatives, particularly her father, not from the boy."

A good example of the sort of pressures which can lead to a development of MCCM is given by Oliver (1955:269). Among the Siuai of eastern Bougainville MCCM "is probably to be explained by the circumstance that some matrilineage property (land and other economically useful resources), although passed from one generation to the next along the female line, tends at any one time to be publicly identified with male members of the matrilineage, with respect to use and disposal. A son-in-law who is also a sister's son is a matrilineage mate to whom a man can safely entrust active proprietorship over matrilineage property, whereas a son-in-law who is merely a wife's brother's son may belong to a different matrilineage or sib." Another example of the same sort of pressures is given by Burling (1958:747) for the Garo: "The young couple which lives with the girl's parents has the responsibility of caring for them when they become old and unable to work. This responsibility is deeply felt, and generally executed. In return, this couple inherits all the physical property of the girl's parents and much of the social position as well." "This would

appear to be the reason why the Garo feel that only a nephew is acceptable as an heir. Any other man, even if a son-in-law, would not be expected to be so responsible. The personal relationships in a family with two adult couples require a degree of cooperation and subordination of personal interests not found in a single nuclear family. If relationships are to be harmonious, the son-in-law must respect the father-in-law sufficiently to carry out the sometimes onerous responsibilities of caring for him in his old age. If the son-in-law is otherwise unrelated to his father-in-law, it is unlikely that he would be willing to subordinate his own desires to those of the older man sufficiently for the maintenance of a harmonious household. If the authority inherent in the relationship between uncle and nephew applies also to father-in-law and son-in-law, the older man will be more willing to exercise the amount of authority necessary for the direction of such a household, and the younger man will be more willing to subordinate his own interests to the general good."

The second process involved in the change of marriage practices would be expected to be concomitant with this development of MCCM. It concerns the exceptions to the developing rule of MCCM. In those cases where a girl does not marry a real or classificatory FSs, the other men married to women of her lineage are likely to exert great influence on her marital choice. That is, if she does not marry into her father's lineage, the other men are all likely to wish her to marry into theirs, assuming these to be different from her father's. Now, let us suppose that at any particular time there are ten men married to the women of a girl's lineage: four men of lineage A, three men of lineage B, two men of lineage C, and one man of lineage D, who is the girl's father. Suppose further that the girl does not marry a man of lineage D. If all the other males have approximately equal influence on her marriage choice the odds are that she will marry a man of lineage A. This means that in the next generation there will be, let us say, five men of lineage A, three men of lineage B, and two men of lineage C. If again there is a girl who does not marry her real or classificatory FSs, and who is in, to take the simplest case, lineage C, the chances are increased that she will marry a man of lineage A. Note also that the exceptions to the MCCM rule are most likely to occur among the daughters of men who come from lineages which are under-represented among the inmarrying spouses, since the strongest pressures towards MCCM will be exerted on the girl whose father has many lineage mates in the residence group. Since the above arguments are independent of the particular number of men in the residence group, granted a long period of time, there will be not only a shift toward increasingly preferential MCCM but also toward the increasing domination of one lineage with respect to the membership of the inmarrying males. As times goes on the situation may approach *prescriptive* MCCM and connubium, a condition in which one lineage gets its wives from a single other lineage and marries its women to the men of a single third lineage.

Let us now consider a partrilineal, patrilocal society practicing BCCM and having symmetrical cousin terms. In such a society the core of the residence

group consists of patrilineally related males; residing with them are their unmarried daughters, and their wives. The latter are, in approximately equal numbers, MBd and FSd, real and classificatory, of the men of the core group. These women are, of course, likely to be drawn from a number of other patrilineages.

Now, suppose that such a society is subjected to pressures which make cooperation among the females of a residence group very important, while other factors require that the residential unity of the patrilineage be maintained. The factors increasing the need for residential cooperation among women could be as many and diverse as those which increase the need for cooperation among men in a matrilineal society. As in the case of the matrilineal society discussed above, there are *in theory* two possible sorts of social change. One would be a shift to amitalocality, the other a change in marriage practices. As opposed to avunculocality, however, amitalocality is not possible. (See our discussion below.) Therefore, in a patrilineal society we would expect the analogues of the two processes discussed earlier to occur. A son is likely to be influenced to choose, or have chosen for him, a girl who is a real or classificatory MBd. The reasons for this are parallel to those discussed in our consideration of the matrilineal society: the women married to the men of a patrilineage will exert pressures upon their sons and other males to marry girls of their own (the women's) patrilineages.<sup>9</sup> As in the earlier discussion it is only necessary to consider one set of parents (in this case, the boy's). Here again, the society may be expected to shift more and more toward connubium and *prescriptive* MCCM.

The above arguments give, it seems to us, a fairly reasonable efficient cause<sup>10</sup> explanation of the development of MCCM. It is accounted for in terms of the desires of individuals to have same-sex members of their own lineages reside with them.<sup>11</sup> This residential proximity is made important by a need for cooperation among individuals of the sex through which descent is *not* reckoned. One can also give a final cause explanation of MCCM. It should be noted that in both matrilineal and patrilineal societies MCCM tends to create a type of residence group which is likely to maximize the possibilities of local cooperation and local action: a residence group which consists of a line of related men and a line of related women. Cooperation is maximized because these related individuals have already learned to cooperate within the residence group of their birth. Action is more effective because the members of each sex can act as a corporate unit. In some of this argument it is clear that we verge on the theories of Levi-Strauss (1949). But where he sees MCCM producing social solidarity through the creation of dependence among kin groups, we see the important factor as the creation of a harmonious form of residence group. The two views are definitely not incompatible. Similarly, Levi-Strauss' notion that PCCM creates a disharmonious regime can be interpreted in another way by pointing out that it does not create a type of residence group in which the members of each sex are lineage mates, since, as has now often been shown (Levi-Strauss 1949:551; Leach 1951:26-8;

Needham 1959:203), PCCM brings women from different lineages in contiguous generations. Again our views are entirely consistent with those of Levi-Strauss but emphasize a different and, we think, more important factor. This point negates, we believe, the argument of Homans and Schneider (1955:13) that PCCM may create more social solidarity than MCCM by virtue of its "roundaboutness" (the fact that the "return of women" is delayed by a generation). In terms of residence, PCCM cannot create more solidarity than MCCM. The difference between MCCM and PCCM is due, of course, to the fact that with MCCM both a male and a female ego obtain a spouse from a parent's sibling who is of the same sex as ego, while with PCCM both a male and female ego obtain a spouse from a parent's sibling who is of the opposite sex. When these facts are combined with rules of patrilocal and matrilocal residence the crucial residential difference between the two forms of unilateral cross cousin marriage is obvious.

Some of the facts which we found the jural authority theory of unilateral cross-cousin marriage unable to explain are consequences of the hypotheses presented so far. First, this theory explains why a certain number of societies should have *prescriptive* MCCM. If we are correct, this is due simply to an intensification and continuation through time of the forces which produce *preferential* MCCM. Second, we can explain why there is no *prescriptive* PCCM. In a unilineal society PCCM produces a residence group which is in conflict with the ideal of maximizing local cooperation. The forces which lead to prescriptive MCCM could never lead to prescriptive PCCM. The factual asymmetry in prescriptive marriage rules is thus a consequence of our account of the origin of MCCM. Third, this theory partially explains why MCCM is relatively rare even in patrilineal societies. We do not attribute MCCM to a general feature of the social structure of such societies. Instead, MCCM is attributed to conditions which must necessarily be fairly rare, conditions in which a unilineal society is subjected to pressures for residential cooperation among individuals of the sex through which descent is not reckoned.

The fourth, and most important, consequence of our assumptions so far is that MCCM will almost never arise in a society with bilateral descent. We attribute the development of MCCM to an increase in the importance of the sex through which descent is not reckoned and to the pressures which this puts upon individuals to influence the marriage choices of their children and the other children of their residence group. In societies with bilateral descent such pressures toward cooperation by persons of one sex would presumably bring about a simple shift in residence, for there can be no need to maintain the residential unity of nonexistent lineages. Even if the above argument did not hold, there are other factors which make MCCM unlikely in bilateral societies. According to the theory presented here, MCCM arises due to the desire of adults to incorporate in their residence groups persons of the same sex with whom they have already established structured relations and habits of cooperation by virtue of common lineage membership. But in a bilateral society there are no groups which would differentiate MBd from FSd with

respect to these factors. In a society with bilateral descent one has identical structural relations with both MBd and FSd and thus there is no basis for the development of MCCM.

There is one further reason why MCCM should not develop in societies with bilateral descent. In a unilineal society MCCM can be generalized as a rule which links kin groups rather than individuals or families, because each MBd or FSs can be structurally equated to the other members of his or her lineage who are of the same generation and sex. In a bilateral society, however, this is obviously not the case and MCCM would remain essentially a rule between individuals. That is, in a bilateral society MCCM would have to mean marriage with the real MBd and only with her. But on biological grounds such a rule is unworkable, for the probability of there being a proper number of eligible MBd and FSs for any large segment of a society is very low at any one time. (Note that with BCCM this probability is doubled.) It thus appears to us that our theory provides a very strong account of why MCCM is almost nonexistent in bilateral societies.

#### V. FREQUENCY

As opposed to Homans and Schneider, we see MCCM arising with no greater frequency in patrilineal than in matrilineal societies. There are, in fact, factors which will make the frequency of development of MCCM greater in *matrilineal* societies. We have said that the development of this marriage rule depends on the increased importance of men in matrilineal societies and of women in patrilineal societies. But it would seem very likely that the former situation would develop more often, and be more pressing when it did develop, than the latter. There is a further theoretical ground for believing that MCCM will develop more often in matrilineal societies. The rise of MCCM depends in matrilineal societies on the influence of *men* over the marriage choices of their daughters and other girls of their wives' lineages, and in patrilineal societies on the influence of *women* over the marriage choices of their sons and other boys of their husbands' lineages. As Homans and Schneider (1955: 27-28) point out, however, a man is likely to have more control over his child's marriage than a woman is likely to have over hers. There is thus a second and independent reason why MCCM should arise in a higher proportion of matrilineal than patrilineal societies.

The reader may, at this point, find our position somewhat confusing since he will see that our theory makes predictions which are in direct opposition to the factual correlation discovered by Homans and Schneider, and confirmed by Murdock, that MCCM is associated with patrilineality. We request his forbearance for a few pages. Before rejecting our theory it is necessary to reconsider the question of Crow-Omaha kinship systems.

#### VI. INCEST

It was shown in section I that MCCM alone is not sufficient to produce the alignments for Crow-Omaha kinship systems, but that an approximation to

connubium is required. We have shown, however, that MCCM and a progression toward connubium will tend to develop concomitantly, since both are a natural result of forces toward local cooperation. Hence in all further discussions we assume that MCCM and some approximation to connubium co-occur and therefore speak only of MCCM.

It will be remembered that the data show that Crow-Omaha kinship systems are not associated with MCCM. It might appear that this fact must lead to the rejection of the Lanes' general hypothesis, that MCCM is a determinant of such terminologies. This rejection would be very unfortunate, however. It is an accepted principle of the study of kinship terminology that kinsmen aligned together in the same descent group tend to be called by the same term (Murdock 1949:52, 125-26). The Lanes have shown that MCCM produces an alignment of kinsmen which should lead to Crow-Omaha terminology if this general principle is correct. It would therefore be very convenient for kinship theory in general if the Lanes' general hypothesis could be reconciled with the facts.

Such a reconciliation is not very difficult to accomplish. The rejection of the Lanes' general hypothesis on the basis of the disproof of their special hypothesis depends only on the assumption that the determinants of a social phenomenon must persist as long as the phenomenon itself. We need reject the Lanes' general hypothesis only if we assume that MCCM must persist after it has led to the formation of Crow-Omaha terminologies. No such assumption need be made, however. Rather, there are important facts which imply the exact opposite. First, there is the well known tendency of Crow-Omaha kinship systems to be highly resistant to change, shown, for example, by the occurrence of Crow systems in patrilineal societies, such as the Sukuma, Mongo, and Malekulans. A second, and much more important reason why the causes we are concerned with would *not* persist as long as their effects is the extension of incest taboos.

It is clear that once the alignment for Crow-Omaha terminologies has been established, not only will extensions of kin terms take place from primary relatives out, but also the extension of incest taboos. As Murdock (1949:303) notes, the principles underlying the extension of incest taboos "are similar to those governing the extension of kinship terms from primary to secondary and remoter relatives. . . ." The result is that they "tend to apply . . . to all relatives called by a classificatory term which includes sexually tabooed primary relatives" (Murdock 1949:287).

Consider a hypothetical matrilineal society which has both MCCM and Crow terminology. A male ego is therefore marrying a woman who is in the same matrilineage as his daughter and whom he calls "daughter." A female ego in such a society is marrying FSs, a man in her father's matrilineage whom she calls "father." Now consider a hypothetical patrilineal society with MCCM and Omaha terminology. Here, a male ego is marrying a woman of his mother's patrilineage whom he calls "mother," while a woman is marrying a man of her



son's patrilineage whom she calls "son." It is obvious that in both types of society MCCM necessitates marriage with a person who is structurally and terminologically identified with a primary relative. Such a situation is clearly unstable. The same forces which have led to the extension of kinship terminology, namely membership in lineages or sibs which include primary relatives of ego, are certain to lead to the extension of incest taboos to the same individuals. (Here and throughout this paper we assume that terminological extensions precede incest extensions.) One is thus led to the remarkable conclusion that MCCM is self extinguishing.<sup>12</sup>

#### VII. IMPLICATIONS OF THE EXTINCTION OF MCCM

The most obvious implication of the extinction of MCCM is that the lack of association between MCCM and Crow-Omaha kinship systems is no real argument against the view that such a marriage rule is a determinant of these terminologies.

A second obvious implication of this extinction is that MCCM will be fairly rare. Once in existence it creates, as it were, the seeds of its own destruction. This adds to the reasons for the rarity of MCCM given in section IV.

We argued earlier that MCCM would tend to *arise* relatively more often in *matrilineal* than in *patrilineal* societies. It seems likely, however, that MCCM will *persist* longer in *patrilineal* societies than in *matrilineal* ones. In the progression of a society practicing MCCM toward increasingly preferential MCCM and toward connubium, there would no doubt be a threshold point beyond which Crow-Omaha terminologies would come into existence and beyond which MCCM would begin to be extinguished. There is good reason, however, to believe that this threshold would be reached more rapidly by *matrilineal* than by *patrilineal* societies. That is, *patrilineal* societies would tend to remain in a pre-Omaha MCCM phase longer than *matrilineal* societies would remain in a pre-Crow MCCM phase, and MCCM would be extinguished more rapidly in *matrilineal* societies than in *patrilineal* ones. The reasons for this are roughly identical with those which we discussed above as causing the more frequent *development* of MCCM in *matrilineal* societies. In *matrilineal* societies the drift toward MCCM is controlled by pressures which increase the importance of male cooperation and by the decisions of men concerning marriage choices. In *patrilineal* societies, on the other hand, the progression is controlled by the increasing importance of female cooperation and by women's control over marriage choices. It is clear, however, that male cooperation is more likely to be a crucial factor in a society than is female cooperation, and it is also clear that men in a *matrilineal* society are going to have much more to say about marriage choices than are women in a *patrilineal* society.

It is obvious that the above arguments explain the association of MCCM with *patrilineal* societies.

There are further implications of the extinction of MCCM. Since we have said that MCCM arises more frequently in *matrilineal* societies, the total of

patrilineal MCCM societies or patrilineal Omaha societies should make up a lesser proportion of all patrilineal societies than matrilineal MCCM societies or matrilineal Crow societies make up of the total of all matrilineal societies. Table 2 makes such a comparison. Our hypothesis is hurt by the fact that many Crow societies are no longer matrilineal and thus cannot be counted.

TABLE 2. FREQUENCIES OF MCCM AND CROW-OMAHA TERMINOLOGIES  
IN MATRILINEAL AND PATRILINEAL SOCIETIES

A. Matrilineal Crow societies	25
B. Matrilineal societies with MCCM	
1. Crow	2
2. Non-Crow	5
C. Total of A plus B2	30
D. Total matrilineal societies	84
E. Ratio of C to D	$\frac{30}{84} = .36$
F. Ratio of A to D	$\frac{25}{84} = .30$
G. Patrilineal Omaha societies	41
H. Patrilineal societies with MCCM	
1. Omaha	5
2. Non-Omaha	29
I. Total of G plus H2	70
J. Total patrilineal societies	248
K. Ratio of I to J	$\frac{70}{248} = .28$
L. Ratio of G to J	$\frac{41}{248} = .17$

Hardly any Omaha societies, on the other hand, are nonpatrilineal. Nevertheless, our hypothesis is confirmed. It also follows from the theory that Crow societies should make up a greater proportion of matrilineal societies than Omaha of patrilineal societies. Again this hypothesis is hurt by the fact that many Crow groups are no longer matrilineal and cannot be counted. It can be seen from Table 2 that this second hypothesis is nevertheless also confirmed. We know of no other view of the origin of Crow-Omaha terminologies which can explain these differences.

#### VIII. AVUNCULOCALITY

It would be only natural if the reader regarded the argument thus far as a rather tenuous set of hypotheses. After all, it has not really been shown that the general hypothesis of the Lanes is valid. It has only been argued that this hypothesis is not necessarily invalid *because* of the lack of association of Crow-

Omaha systems with MCCM and that this hypothesis can explain some otherwise obscure facts. What is clearly required to support the foundations of our argument is some independent support for the Lanes' general hypothesis.

Since we have hypothesized that MCCM would be automatically extinguished, it is necessary, in order to test the hypothesis, to find some factor which is related to MCCM and which would not necessarily be extinguished along with MCCM. For Crow societies we believe there is such a factor. This depends on the fact that, with MCCM, matrilocality and avunculocality are identical. That is, matrilocality with MBd is identical with residence with MB. This is noted by Murdock (1949:71; 1959:301). Since this identity holds, as the drift toward MCCM and connubium takes place in a matrilineal society, that society may increasingly be described as avunculocal. Since it was pressures toward male cooperation that brought about the drift to MCCM; and since avunculocality is a perfect solution to the needs created by these pressures, the members of the society may very well come to phrase the rule of residence in terms of avunculocality and to reside avunculocally in exceptional non-MCCM marriages. (Note how easily avunculocality can develop from matrilocality with the drift from BCCM to MCCM. Even with BCCM some men necessarily reside avunculocally. The transition to MCCM merely intensifies this situation. We therefore suspect that most societies which have a rule of avunculocal residence have developed it by way of MCCM or else have borrowed this residence rule from a neighboring group who developed it through MCCM.) In view of these considerations, if the hypothesis that MCCM is a determinant of Crow systems is true, Crow systems should occur more frequently in societies which are avunculocal than do other systems of terminology. That is, within the universe of societies which are matrilineal or are derived from a previous matrilineal phase, Crow terminology should generally be associated with avunculocality, while other systems of terminology should be associated with matrilocality.

Avunculocality and matrilocality are, of course, both associated with matrilineal societies or societies recently derived from a matrilineal phase, except where they occur in bilateral societies. In bilateral societies which are matrilocal we are usually dealing with groups which are shifting to matrilineality. Since avunculocality can only develop out of matrilocality (Murdock 1949:207), it would be unreasonable to include bilateral societies in a test of the hypothesis that Crow systems are associated with avunculocality. We shall therefore compare all Crow societies which have any avunculocality or matrilocality (including the Pawnee, who have bilateral descent) with all non-Crow matrilineal, double descent, or patrilineal societies which have any matrilocality or avunculocality at all.<sup>13</sup>

Of the 30 non-Crow avunculocal societies in the *World Ethnographic Sample* which meet these requirements, five have MCCM. It seems to us quite unreasonable to count these societies *against* the hypothesis, since the theory in fact predicts that there should be such societies, namely, those that have

TABLE 3. CROW TERMINOLOGY AND AVUNCULOCALITY

	Avunculocal	Matrilocal	Totals
All Crow societies	15	12	27
All other non-bilateral societies	25	37	62
Totals	40	49	89
Coefficient of association =	.31		
Chi <sup>2</sup>	= 1.21		
P	= n.s.		

adopted the marriage rule and are on the way to becoming Crow. We have therefore deleted these from the test of the hypothesis in Table 3.

It must be admitted that the coefficient is not overwhelming, nor is it statistically significant. Nevertheless the results are in the direction predicted. We would not expect the coefficient to be 1.00 since, as noted above, we know that avunculocality is borrowed, and because there are other ways in which Crow terminology can develop which do not lead to an association with avunculocality. (The latter will be discussed below.)

Moreover, it should be noted that of the 25 non-Crow societies which are avunculocal, and therefore count against our hypothesis, 8 are Southern and Central Bantu groups and 11 others are in Northern and Central Africa. The avunculocal Crow societies, on the other hand, are much more evenly spread throughout the world. Therefore, if it is accepted that a unique historical occurrence has led to widespread avunculocality in Africa, our results are much stronger than might appear from the statistics alone.

#### IX. PCCM

With one exception,<sup>14</sup> every Crow society in the *World Ethnographic Sample* which allows any first cross-cousin marriage is at least partly avunculocal. As is shown in Table 4, there is a high association within the universe of non-bilateral societies which allow first cross-cousin marriage and which are either

TABLE 4. NON-BILATERAL SOCIETIES WHICH ALLOW ANY CROSS-COUSIN MARRIAGE

	Avunculocal	Matrilocal	Totals
Crow societies	11	1	12
Other societies	20	18	38
Totals	31	19	50
Coefficient of association =	.81		
Chi <sup>2</sup>	= 4.35		
P	= .05		

matrilocal or avunculocal, between avunculocality and Crow terminology. Furthermore, the results are statistically significant. As we have noted, in a matrilocal society MCCM automatically produces avunculocality. It only remains to be explained why there should be such a high frequency of avunculocality in Crow societies which have BCCM or PCCM.

Let us consider a hypothetical Crow society with preferential MCCM. Pressures for a cooperating group of male relatives were the cause of MCCM. But once avunculocality is established, this need is taken care of by the residence rule. Therefore, once avunculocality is established, the pressures toward MCCM are neutralized. No further drift toward MCCM should occur. As a corollary of this, we would not expect any matrilineal societies ever to develop *prescriptive* MCCM. (This is not testable on the basis of the data in the *World Ethnographic Sample*, but we have never heard of a matrilineal society with *prescriptive* MCCM.) It also follows that such Crow societies can shift from MCCM without destroying the line of related cooperating men, MB, ego, Ss, and, therefore, without destroying the residential unity of the lineage.

As we have seen, just such a shift in marriage rule will be required. The incest taboo will gradually extend to MBd and FSs. Since with *preferential* MCCM FSd and MBs are both *possible* marriage partners, as MCCM is extinguished there will be more and more marriages with them. (Note that in a typical Crow system neither FSd, for a man, nor MBs, for a woman, is equated with a primary relative. Therefore, these types would not be banned by the same incest extensions as apply to MBd and FSd.) This will lead to a shift back to BCCM and eventually to PCCM. There may, for a while, be continued marriage with second and third cross-cousins in the same lineage as MBd, for, although the incest taboo will extend to them, it will do so more slowly and with less strength. Eventually, however, the incest taboo plus a preference for marriage with close consanguineal kin will extinguish marriage with these distant cross-cousins entirely. Since we would expect the extension of the incest taboo to MBd to take place fairly soon after the terminological extensions, it is entirely reasonable that the phases of BCCM and PCCM would be reached before any factors could lead to a loss of avunculocality. In time, of course, maximal extension of incest taboos will tend to prohibit all first cross-cousin marriage.

We thus suggest that there is a normal progression in many Crow societies from *preferential* MCCM to BCCM to PCCM and finally to a prohibition of all first-cross cousin marriage. This explains not only the association between Crow terminology and avunculocality shown in Table 4 (and Table 3 as well), but also explains why any Crow societies should have any BCCM or PCCM at all. The occurrence of these marriage rules with Crow terminology is not explained by any other theory we know of.

There is another bit of evidence to support the hypothesized progressions. There are 8 matrilineal societies in the *World Ethnographic Sample* which have PCCM. Four of these 8 have Crow kinship systems. Table 5 shows the relevant data. Within the universe of matrilineal societies there is an association

between Crow terminology and PCCM. Because of the very low numbers, however, it is not statistically significant. Our theory predicts the observed trend by making such a rule of marriage a definite phase in the development of many Crow societies.

Further support for the suggested progression in Crow societies is found in the fact that every phase of the theoretical progression can be illustrated by some real society represented in the *World Ethnographic Sample*.

Phase 1. Matrilineal descent, symmetrical cousin terms, BCCM, matrilocal:

Carrier	Vedda
Guahibo	Yaruro
Nauruans	

Phase 2. Matrilineal descent, symmetrical cousin terms, MCCM hence avunculocal:

Garó	Belu
Chokwe	Siuai
Lamba	

Phase 3. Matrilineal descent, Crow cousin terms, MCCM hence avunculocal:

Kaska	Mnong Gar (not in sample) <sup>15</sup>
Siriono	

Phase 4. Matrilineal descent, Crow cousin terms, BCCM, avunculocal:

Kongo	Ponapeans
Ndoro	Goajiro

Phase 5. Matrilineal descent, Crow cousin terms, PCCM, avunculocal:

Lobi	Haida
Trobrianders	Tlingit

The following *matrilocal* Crow societies in the *World Ethnographic Sample* are not explained by the theory being presented here:

Mandan	Creek	Zuni
Pawnee	Timucua	Bororo
Cherokee	Acoma	Timbira
Choctaw	Hopi	Trukese

In addition, the Wappo are a completely anomalous Crow group, having bilocal residence and bilateral descent.

These societies can not readily be seen as part of the sequences being discussed in this paper and must be accounted for on other grounds.<sup>16</sup> Many of them may be the result of the operation of reciprocity and analogy as discussed by Murdock (1949:166-67): ". . . under matrilineal descent, FaSi and her children are members of the same consanguineal kin group; the fact of such participation presumably operates . . . to produce an extension of kinship terminology from the first ascending generation to lower generations within the same kin group. Once this happens, the reciprocal terms BrSo and BrDa are

extended to the other cross-cousins, the children of MoBr, both through analogy and by virtue of the fact that such relatives are all children of the men of Ego's own matrilineage or matri-sib."

The Crow terminologies in some of the societies not explained by MCCM might also result from a rule of preferential marriage with a second or third cross-cousin who is in the same lineage as MBd. Such a rule in conjunction with connubium will, in both patrilineal and matrilineal societies, produce an alignment of the diagnostic types for Crow-Omaha systems, but will not, of course, produce avunculocal residence in a matrilineal society. In this regard, it is interesting to note that the Cherokee are reported by Gilbert (1937:283-338) to have had preferential marriage with FFSdd and that the same marriage rule is reconstructed by Lounsbury (1956:182-83) for the Republican Pawnee. With such a marriage rule, FFSdd is identical with MMBBddd.<sup>17</sup> It is to be expected, of course, that even a rule of third matrilineal cross-cousin

TABLE 5. MATRILINEAL SOCIETIES WITH PCCM

	Crow	Non-Crow	Totals
PCCM	4	4	8
PCCM absent	18	47	65
Totals	22	51	73
Coefficient of association =	.45		
Chi <sup>2</sup> =	.78		
P =	n.s.		

marriage would eventually extinguish itself by creating the basis for incest extension.

Our theory assumes that the shift to BCCM and PCCM in a matrilineal society is made possible by the fact that such a society has developed avunculocality. There is one type of avunculocality which would not only permit the shift through BCCM to PCCM, but would positively encourage it. Should an avunculocal matrilineal society with MCCM develop a rule of premarital avunculocality, in which a boy goes to live with his MB at an early age, the incest taboo would probably very rapidly be extended to MBd with whom the boy is living. The result would be that the shift to BCCM and then PCCM would develop very rapidly indeed.

We do not maintain that pre-marital avunculocality must develop, but only that it may. Pre-marital avunculocality is rare. We have, however, theorized that MCCM will develop in those matrilineal societies where the importance of male cooperation is great, and this is just the type of situation in which pre-marital avunculocality is most likely to be adopted. For if a structured relationship between MB and ego and between ego and Ss is the

basis of local cooperation, it is clear that it will be advantageous for a man to have his Ss residing with him from an early age. This will reinforce the relationship which is already inherent in the nature of the lineage group and will forestall any possible antagonisms over authority which might otherwise occur. In fact, three well known Crow societies have pre-marital avunculocality: the Haida, the Tlingit, and the Trobrianders.

The above deals with societies having MCCM which adopt premarital avunculocality. In matrilineal societies with BCCM which adopt such a rule the results would be quite different. Here there would be no MCCM phase and thus no development of Crow terminology. But the society should still shift toward PCCM, since MBd would again become tabooed.

If the above two related processes do account for many of the cases of PCCM, there should be an association between PCCM and avunculocality. Table 6 shows the data relevant to this prediction. As the table shows there is a remarkable association between PCCM and avunculocality, which is highly significant statistically. This offers, we think, strong support for our view of the origins of much of PCCM.

TABLE 6. PCCM AND AVUNCULOCALITY

	Avunculocal	Non-Avunculocal	Totals
PCCM	10	6	16
PCCM absent	42	506	548
Totals	52	512	564
Coefficient of association =	.91		
Chi <sup>2</sup>	= 49.17		
P	= .001		

In summary, we think that most cases of PCCM develop from the prohibition of marriage with MBd in societies which prefer marriage with MBd or with both MBd and FSd. This prohibition is due to incest extensions which result from *either* or both of the following two factors:

1. Pre-marital avunculocality,
2. The kin alignments produced by MCCM.

When the first of these occurs in a society with BCCM it produces a shift to PCCM. When pre-marital avunculocality occurs in societies with MCCM it reinforces the self extinguishing character of MCCM.

#### X. THE PROGRESSION IN OMAHA SOCIETIES

In the last section we discussed the developments which would take place in a Crow society given the progressive spread of incest taboos to MBd and FSs. Here we consider what happens in an Omaha society when the same incest taboo extensions occur.

There is a crucial difference between a MCCM-patrilineal, patrilocal so-



ciety and a MCCM-matrilineal, avunculocal society with respect to the progressions we are considering. The forces which bring about MCCM are pressures for male cooperation in a matrilineal group and for female cooperation in a patrilineal group. Once established, the rule of avunculocality, phrased as such and governing exceptions to the marriage rule, permits a matrilineal society to shift its rule of marriage without losing its line of related males. (And this shift becomes necessary because of the extension of incest taboos.) The rule of residence in a patrilineal society parallel to avunculocality in a matrilineal society is amitalocality, in which a couple goes to live with the wife's FS upon marriage. Just as MCCM produces an identity of matrilocality with avunculocality in a matrilineal, matrilocal society, it produces an identity of patrilocality with amitalocality in a patrilineal, patrilocal society.

Once established, such a rule of amitalocality, phrased as such and governing exceptions to the marriage rule, would permit a patrilineal society to shift its rule of marriage without losing its line of related females. But such an independent rule of amitalocal residence is, of course, unknown (Murdock 1949: 71). This is no doubt due to the fact, pointed out by Goodenough (1951:427-429), that a shift to amitalocality would involve all of the disadvantages, noted by Murdock (1949:213-18), entailed by a shift from patrilocality to matrilocality. (Homans and Schneider, 1955:53-55, also give good reasons for the nonexistence of amitalocality.)

Therefore, as long as the pressures for a local group of related females continue in a patrilineal society, the drift toward MCCM and connubium will continue. From this we can deduce two conclusions. First, despite the fact that a patrilineal society moves more slowly toward *prescriptive* MCCM than a matrilineal society, it is likely to reach such a phase of *prescriptive* MCCM while, as noted above, a matrilineal society will not do so. Second, a patrilineal society is very unlikely to make a shift to *preferential* BCCM and PCCM.

A shift to BCCM and PCCM should occur very rarely in patrilineal societies for the following reasons. First, if a patrilineal society has reached a phase of *prescriptive* MCCM, such a shift is unlikely because FSd and MBs have been *prohibited* as potential spouses. (Note that such an argument does not apply to matrilineal societies because of the absence of *prescriptive* MCCM. Second, where MCCM begins to be extinguished before it has become *prescriptive*, the tendency to shift to BCCM and PCCM should be overcome by the continued pressures toward maintaining a line of related females in the residence group. As we have seen, the analogous problem in the matrilineal societies is solved by the institutionalization of avunculocality, but patrilineal societies cannot institutionalize amitalocality.

It is, therefore, the fundamental lack of symmetry between avunculocality and amitalocality which accounts for the fact that matrilineal MCCM societies tend to shift through BCCM to PCCM while patrilineal societies do not. In patrilineal societies we would expect that, since marriage with real MBd has been extinguished by incest taboo extensions, preferred marriages would be with second and third cross-cousins who are in the same lineage as

MBd. The second and third cousin types would have been marriageable under the MCCM rule. Since they are in the same lineage as MBd, marriage with them constitutes at least a temporary and partial solution to the dilemma in which the members of a patrilineal Omaha society find themselves. As Murdock (1949:286) notes: "Of the handful of ethnographers who give adequate information on the differential intensity of incest taboos, all report for their respective tribes that, for example, the taboos apply more strongly to own than to 'classificatory' sisters, to half sisters than to cousin, to first than to second or remoter cousins." Thus an Omaha group can, for a time, avoid the incest taboo by prohibiting MBd but by still marrying more distant cross-cousins who are in the same lineage as she, even though these more remote cousins may also be called "mother." (The same considerations hold for a female ego, who will be marrying more remote cross-cousins who are in the same lineage as FSs.) This retreat to second and third cross-cousins in the same lineage as MBd will maintain the line of related females in the residence group.

These considerations lead one to expect that many of the Omaha societies which prohibit first cross-cousin marriage have preferred marriage with second or third cousins who are in the same lineage as MBd. (Unfortunately, this is not testable on the basis of the data in the *World Ethnographic Sample*.) Eventually, of course, the incest taboo will extend to these more remote cousins. Incest taboos, as it were, chase matrilineal cross-cousin marriage to more and more remote relatives, eventually, presumably producing its extinction.

The postulated shift to *preferred* marriage with second and third cousins in the same lineage as MBd obviously permits an Omaha society to pass through phases of *allowed* BCCM and *allowed* PCCM. If Omaha terminology has developed on the basis of a *preferential* rule of MCCM (rather than prescriptive), as MBd is slowly banned by incest extensions, there should be phases during which, first, marriage with *both* MBd and FSd is *allowed*, and then, marriage with FSd *only* is *allowed*. But the continued operation of pressures toward a co-resident line of related females will ensure a continued drift towards marriages with members of MBd's lineage, so that marriages with FSd will become more and more rare. Thus, cases of Omaha societies with *allowed* BCCM should be rare, and cases with *allowed* PCCM extremely rare. There are, in fact, three Omaha societies in the *World Ethnographic Sample* which have *allowed* BCCM, the Ostyak, the Yakut, and the Winnebago. There is one society in the *World Ethnographic Sample* listed as Omaha which has *allowed* PCCM, the Sherente. We do not consider the Sherente to be an example of an Omaha society which has shifted to PCCM, however. The Sherente have an extremely anomalous kinship system and are not in any sense typical Omaha. It seems almost certain that they retain PCCM from a previous Crow stage interrupted by the development of patrilineality (Murdock 1949:334). Our theory obviously predicts that no Omaha societies should have *preferential* BCCM or PCCM. There is only one exception to this in the *World Ethnographic Sample*, the

Uzbek. *Preferred* BCCM among the Uzbek, like *allowed* BCCM among the Yakut and Ostyak, is, we suspect, related to the fact that all of these societies allow some parallel cousin marriage.

We can now understand the rarity of PCCM in general, its relative rarity as compared with MCCM and, even more importantly, its association with matrilineality. PCCM is associated with matrilineality because it develops for the most part as a result of the extinction of marriage with MBd. This occurs under the following circumstances:

1. when a matrilineal society with BCCM adopts premarital avunculocality,
2. when marriage with MBd becomes prohibited in a matrilineal society with MCCM.

Both of these circumstances require a viable rule of avunculocality. Logically, PCCM should arise in patrilineal societies under the following analogous circumstances:

1. when a patrilineal society with BCCM adopts pre-marital amitlocality,
2. when marriage with MBd becomes prohibited in a patrilineal society with MCCM.

But neither of these theoretical possibilities actually leads to PCCM in patrilineal societies because both require amitlocality, which is not a workable form of residence. This explains why PCCM is both a rare phenomenon in general and rare as compared to MCCM.

We here end our account of unilateral cross-cousin marriage. In contrast to the symmetrical jural authority theory of Homans and Schneider, which views MCCM and PCCM as essentially equivalent sorts of institutions, ours is a distinctly asymmetrical explanation. PCCM is seen as an anomaly due to incest extensions which occur under certain limited conditions. MCCM, on the other hand, is a reasonable response to the problems of residential cooperation in unilineal societies.

#### XI. CONCLUSION

In this paper we have presented a theory of the origins of the different forms of unilateral cross-cousin marriage and of Crow-Omaha kinship systems. The crucial bridge between these two sets of phenomena is the important discovery, somewhat in need of modification, made by Lane and Lane that MCCM will produce the necessary alignment of kin types for the development of Crow-Omaha systems.

The factual support for this theory has come from the selected data in the *World Ethnographic Sample*. In its major outlines the theory is strongly supported by these data. A large number of points have been assumed or inferred, however, which can be checked by analyses of ethnographies and by field work. This should, of course, be done.

In its present form we believe this theory explains all of the following facts:

1. Crow kinship systems exist.
2. Omaha kinship systems exist.
3. Crow kinship systems are associated with matrilineality.
4. Omaha kinship systems are associated with patrilineality.
5. MCCM exists.
6. PCCM exists.
7. MCCM is associated with patrilineality.
8. PCCM is associated with matrilineality.
9. MCCM and PCCM are almost nonexistent in bilateral societies.
10. Crow kinship systems are not associated with MCCM.
11. Omaha kinship systems are not associated with MCCM.
12. MCCM is rare.
13. PCCM is rare.
14. PCCM is rarer than MCCM.
15. Prescriptive MCCM occurs.
16. Prescriptive MCCM does not occur in matrilineal societies.
17. Prescriptive PCCM does not occur.
18. Among societies with Crow kinship systems, those which allow some kind of first cross-cousin marriage are almost invariably avunculocal.
19. Crow kinship systems are associated with avunculocality.
20. Matrilineal Crow systems are associated with PCCM.
21. PCCM is associated with avunculocality.
22. A higher proportion of matrilineal societies have Crow kinship systems than patrilineal societies have Omaha kinship systems.
23. The ratio of MCCM or Crow matrilineal societies to the total of matrilineal societies is greater than the ratio of MCCM or Omaha patrilineal societies to the total of patrilineal societies.
24. Some Crow societies have MCCM, some BCCM, and some PCCM.
25. Some Omaha societies have MCCM, few BCCM, and hardly any PCCM.

## NOTES

<sup>1</sup> This paper is a joint effort. Our names are listed in alphabetical order. We would like to express our special thanks to Professor George Peter Murdock, without whose aid and encouragement this paper would not have been written. It would be difficult to exaggerate our indebtedness to him, not only with respect to his published work, but also with respect to the many fruitful ideas he has provided us in long hours of personal discussion. This is not to say, however, that he necessarily endorses all that we say here. We should also like to express our thanks to Professors Floyd Lounsbury and Leopold Pospisil, each of whom read an earlier version of this work and each of whom made many helpful criticisms. We are also indebted to Manindra K. Nag for advice with regard to statistical questions. Needless to say, none of the above are responsible for any shortcomings or errors in this work, which are the responsibility of the authors alone. Mr. Eyde's part in this investigation was supported by a research fellowship (No. 10,357) from the National Institute of Mental Health, Public Health Service.

<sup>2</sup> We use the terms "Crow" and "Omaha" to refer exclusively to the systems of terminology and not to refer to any other aspect of social organization.

<sup>3</sup> We have compiled all tables from a corrected version of the *World Ethnographic Sample*.

The corrections were obtained from Professor Murdock in the form of *Addenda and Corrigenda*. They do not make any substantial difference in our results. We have made three changes ourselves. The Apinaye, listed as having Crow cousin terms, are actually Eskimo. The Siriono are listed as having bilateral descent, but we consider them to be matrilineal. (See note 6.) The Kaska are listed as having no other kin groups than matri-moieties. Eggan (1955:543), however, has suggested that this group also had matrilineages. We assume that he is correct.

We use Yule's coefficient of association and Yates' correction in computing  $\chi^2$ .

<sup>4</sup> Following the practice of Homans and Schneider (1955:32-33) and the *World Ethnographic Sample* (Murdock 1957:672), we define the forms of first cross-cousin marriage in terms of verbal rules rather than marriage statistics. Many marriages in societies with such verbal rules will be with second and more removed cousins. This is still to be considered a rule of first cross-cousin marriage as long as the rule is stated in terms of first cousins. First matrilineal cross-cousin marriage will be abbreviated MCCM, first patrilineal cross-cousin marriage will be abbreviated PCCM, and first bilateral cross-cousin marriage will be called BCCM. Other marriage rules will not be abbreviated.

<sup>5</sup> This assumes, of course, that moiety organization is not present or, if it is present, that there are, in addition, sibs or lineages which regulate marriage. In all of our arguments throughout this paper we assume that these conditions are met.

<sup>6</sup> These 4 are the Turks, the Callinago, the Panare, and the Siriono. The Turks do not prefer, but only *allow*, PCCM, in conjunction with marriage with FBd. This is no doubt a side effect of the Moslem emphasis on marriage with patrilineal relatives. Since the publication of the *World Ethnographic Sample*, Taylor (1957:290) has indicated that, without being able to prove it, he strongly suspects that the Callinago had matrilineages. Also, in material published after the *World Ethnographic Sample*, Wilbert (1959) has indicated that the Panare practice BCCM rather than MCCM, as had previously been reported by Riley (1954).

If these modifications and qualifications are accepted, the Siriono are the only society in a sample of 204 which violate the rule that preferential unilateral cross-cousin marriage does not occur in societies with bilateral descent. Because of this uniqueness and because there are strong theoretical grounds for believing that *preferential* unilateral cross-cousin marriage can only arise in unilineal societies, we feel that the Siriono must be matrilineal, since their marriage rule appears indubitable. The presence of Crow terminology supports this conclusion. We therefore take the always dangerous step of disregarding the published ethnography and consider the Siriono as matrilineal in this paper.

<sup>7</sup> The rarity of unilateral cross-cousin marriage in bilateral societies was pointed out to us by Professor Murdock and has been noted by Needham (1956:108).

<sup>8</sup> The Lanes (1959:262) suggest that the development of MCCM may be due to the asymmetrical extension of incest taboos in societies with BCCM. They do not elaborate this suggestion. We do not see how this could be the case. Attempts to explain MCCM on such a basis often ignore the point of view of a female ego. For further discussion of this point see footnote 12.

<sup>9</sup> Leach (1951:36-37) rejects such an argument for a patrilineal society as nonstructural and as involving unanalyzed psychological assumptions. The argument presented here certainly involves psychological assumptions, but these appear to us to be entirely reasonable. We do not see that the argument is nonstructural. It would be very strange indeed if unilineal kin groups did not channel interpersonal relations, and if individuals did not feel closer to members of their own lineage than to individuals outside of it.

<sup>10</sup> For a discussion of the efficient cause-final cause distinction see Homans and Schneider (1955:14-20).

<sup>11</sup> This is entirely consistent with the strong emphasis of Leach (1951:passim) that MCCM is a relationship between residence groups (local descent groups in his terminology).

<sup>12</sup> Titiev (1956:859) has argued in a way which contradicts this point. He asserts that if unilateral cross-cousin marriage occurs in a patrilineal Omaha society, it must be with MBd, because marriage with FSd would involve marriage with a child of one of ego's "natal kin," that is, a relative (FS) who resides in the residence group into which ego is born. This seems to ignore the fact that MCCM involves marriage with FSs by a female ego. FSs is as much the child of one of a

woman's "natal kin" as FSD is the child of a man's. Titiev's argument that patrilineal cross-cousin marriage must go with matrilineal Crow systems breaks down in the same way.

<sup>13</sup> Throughout this paper, a society which has any avunculocality at all, even as an alternate rule of residence, is counted as avunculocal. A society which is not avunculocal by this definition and has any matrilocality at all is counted as matrilineal.

<sup>14</sup> This is the Bororo. Note that they fail to meet the conditions of footnote 5.

<sup>15</sup> Since we have interpreted both of the MCM-Crow societies (Kaska and Siriono) somewhat differently from the original ethnographies, we have inserted one indubitable example of this phase which is not included in the *World Ethnographic Sample*. For the data see Condominas (n.d.).

<sup>16</sup> Professor Murdock has pointed out to us that, on distributional and other grounds, all of these societies probably have a bilateral Hawaiian background in which all first cousin marriage was prohibited.

<sup>17</sup> Since FFF = MMBB, FFS = MMBd, therefore FFSdd = MMBddd.

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## *Ethnic Relations in Southeastern Mexico*<sup>1</sup>

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**A**LTHOUGH Spanish colonization of the highlands of Chiapas dates from the early 16th century, the region still constitutes one of the major Indian enclaves within Mexico. The state of Chiapas in southeastern Mexico is located southwest of the Yucatan peninsula and west of Guatemala. The town of San Cristobal de Las Casas, founded in 1528, is the major center of Spanish culture in the highlands of the state. Located at 7000 feet of elevation along the Pan-American Highway, San Cristobal had a population of 17,473 at the time of the 1950 Census (Vivo Escoto 1959:217). The climate is tropical as far as the alternation of rainy and dry seasons is concerned, but quite cool due to the elevation.

Town dwellers are almost all hispanicized to the extent of speaking only Spanish and dressing much as in the rest of Mexico. The inhabitants of San Cristobal call themselves *ladinos*.<sup>2</sup> Genetically they range from pure or almost pure Spanish stock to pure American Indian stock. While the upper class (called *clase alta*, *gente bien*, *la crema*, or *los blancos*) is mostly White, the middle class (*clase media*) is mostly light *mestizo* (mixed blood), and the lower class (*gente humilde*) is in majority dark *mestizo* or Indian-looking. Though an occasional lower class person is Caucasoid-looking, and some upper class persons are somewhat *mestizoized*, the genetic continuum overlaps greatly with the social class continuum. This is not to say, however, that genetic characteristics are the primary criteria of social status in San Cristobal. While the town is rigidly stratified, and its inhabitants strongly class-conscious, wealth (as indexed by dress, landownership, type of house, number of servants, etc.), and education (as shown by literacy, correctness of speech, university degree, manners, etc.) are more important than physical appearance in determining one's status. An educated person of Indian appearance may be the object of mild ostracism from the old upper class families but is accepted among the middle class. Conversely, a white skin will not exempt a poor illiterate person from membership in the lower class.

The social class system is reflected in the ecology of San Cristobal: the social status of houses is inversely proportional to the distance from the central town square. As one walks from the center of the town to its outskirts, houses change from bricks or stones and tile roofs, to whitewashed adobe and tiles, to raw adobe and tiles, to straw-thatched huts with lattice walls. Street condition deteriorates from concrete, to irregular cobblestones, to unimproved mud holes, with the exception of one main arterial street that crosses the city from east to west. The arrangement of the San Cristobal cemetery, located



southwest of town, likewise reflects the social status of the defunct inhabitants. While the central path is bordered on both sides by elaborate chapel-like family vaults, the fringes of the cemetery consist of unkept, weed-invaded earthen mounds with decayed wooden crosses. Small stone monuments and fenced-in wrought-iron crosses occupy the intermediate zone between the center and the periphery.

Surrounding the ladino town for a depth of some 40 miles in all directions is a rural hinterland, inhabited by an estimated 125,000 Indians belonging to two major linguistic groups (Tzotzil and Tzeltal, both of which belong to the Mayan family), and subdivided into several smaller ethnic groups (Chamulas, Zinacantecos, Huistecos, Tenejapanecos, etc.).<sup>3</sup> While small ladino nuclei live in the countryside, the overwhelming majority of the rural population is Indian. A number of Indians live in the lower class outskirts of San Cristobal, but in most cases they become rapidly ladinoized. The same is true of Indian girls serving as maids to ladino families in the town. In general, Indians are rural and ladinos are urban. Indians outnumber all ladinos (i.e., rural and urban together) by about six or seven to one. This compares with an Indian minority of around 15 percent for Mexico as a whole. Ever since the Spanish conquest in the 16th century, these Indians (called "indios," "indtgenas," or more frequently in the diminutive form "inditos," by the ladinos) have been subjugated to, and living on the margin of, the dominant Hispanic society. To this date, the Indians have remained, in quasi-totality, small-scale peasants subsisting on a maize-and-beans diet. They have retained their own cultures, wear distinct costumes, are overwhelmingly illiterate, and speak little, if any, Spanish. The 1950 Census lists several Indian districts as having less than 5 percent of their population literate. In the majority of the Indian districts the percentage was under 20 percent. In 1954, the Instituto Nacional Indigenista lists only 4,588 Indians as literate in the region, i.e., some 4 percent of the total Indian population (Villa Rojas 1959:2). At the time of the 1950 Census, in 7 of the 15 Indian *municipios* over 80 percent did not speak any Spanish, and in 12 of 15 *municipios* there were over 60 percent non-Spanish-speaking people. In the 1940 Census, of 79,849 Indians over 5 years of age, 56,523 are classified as non-Spanish-speaking (Villa Rojas, 1959:4; Vivo Escoto 1959:237-45).

In contrast to ladino society, the various Indian ethnic groups show relatively little internal stratification, but the ladinos rank the several Indian groups in a definite hierarchy with the Zinacantecos on top, the Tenejapanecos and Huistecos at the bottom, and Chamulas and other groups in an intermediate position. To some extent this hierarchy is also recognized by the Indians themselves, at least by the Zinacantecos.<sup>4</sup>

The superiority of the ladino group over all Indian groups is taken completely for granted by the ladinos, even the most liberal ones, but the rationalization of that superiority is almost always cultural. Besides strictly cultural differences between ladinos and Indians, economic and educational differences also contribute to the devaluation of the Indians. On the whole, Indians are

poorer and less educated (in the Western sense of the word) than the ladinos, though not always so compared with some lower class ladinos. Racialism, in the form found in South Africa or the United States, is either completely absent, or present in such an attenuated form as to be unrecognizable. The distinction between ladinos and Indians is cultural rather than racial. After four centuries of subjugation, the Indians, on their side, seem to have accepted, and even internalized, the ladino scale of cultural evaluation, though not without ambivalence.

Since the ladinos and the Indians belong to widely different cultures, it is clear that the basic values of the two groups have a bearing both on each group's self-image and on their mutual relationships. While ladino culture is vertically structured and stresses competition and command-and-obey relations, Indian culture is basically horizontal.<sup>5</sup> In spite of the importance of age and of rigid etiquette based on relative age of the participants, Indian culture deemphasizes social hierarchy. Authority is exercised through persuasion and influence rather than through commands. The holding of *cargo* (religious office) in Indian groups is more of a leveling force than one making for hierarchy; for, while *cargo* confers prestige, it also entails the spending of large sums of money for entertainment, thereby preventing the concentration of wealth (Vogt 1959). Witchcraft and the fear of witchcraft also act as powerful leveling forces in the Indian cultures. Ladino society, on the contrary, is highly status-conscious and strongly stratified along class lines. Command-and-obey relations are stressed in the family, the school, the political framework, and the work situation. This basic difference in values may account, in part, for the unquestioned assumption of cultural superiority on the part of the ladinos who view the cultural differences in clearly hierarchical terms. On the other hand, the horizontal stress of the Indian cultures militates against the acquisition by individual Indians of the necessary skills and resources to "pass" as ladinos.

Another important aspect in which Indian and ladino values are at odds concerns the attitude towards manual labor. Whereas ladinos of all classes deprecate manual labor and go to great length to avoid it (even though lower class ladinos are often unsuccessful in their attempt to do so,) Indians value manual work and skills as the most worthy of esteem. A "good" man is one who is skilled in the planting of maize. A "good" woman is one who is a proficient weaver and housewife. As the Indians do, in fact, most of the manual labor (at least in agriculture)<sup>6</sup> and ladinos have a quasi-monopoly of non-manual labor, it is clear that such values tend to reinforce the existent division of labor, to lower the ladinos' estimate of the Indians, and to discourage the Indians from rising to nonmanual occupations (since doing so might involve a loss of esteem on the part of other Indians).

Though both groups are nominally Catholic, Indian religion is actually a syncretism of Mayan and Christian beliefs, whereas the ladinos practice a more orthodox Roman Catholicism. The elaborate *cargo* system that plays such an important role in Indian culture is nonexistent among ladinos. Under

the cover of the Catholic saints, the Indian view of the divine is definitely polytheistic, whereas ladino Catholicism is more monotheistic. The Indian conception of the soul is much more complex and pluralistic than the ladino one (Vogt 1960). Whereas, in their relation to the divine, ladinos seek to maintain or restore what they consider to be the proper personal relation between themselves and God, the Indian emphasis is on maintaining harmony and integrity with a pluralistic and unpredictable universe. The Indian practice of religious curing ceremonies under *curanderos* has no counterpart in ladino religion. The revivalistic cult of the "talking saints" is almost exclusively limited to the Indians (Thompson 1954). Ladino spiritualists engage in unorthodox medical practices but in quite different form from the Indian *curanderos*. A talking saint in Soyalo is owned and visited by ladinos but, in general, the practice is confined to Indians. Such differences in religious values, beliefs, and practices affect interethnic relations insofar as the ladinos view the Indians as pagan practitioners of an idolatrous, superstitious, and corrupt form of Catholicism. However, over and above these differences, the formal structure of the Catholic Church is one of the main integrating forces between the two ethnic groups, as we shall see presently.

These value differences between ladino and Indian (more specifically, Zinacanteco) culture, as well as other value differences not so directly related to ethnic relations between the two groups, are summarized in Table 1.

Before proceeding to a more detailed description of the system of ethnic relations in the highlands of Chiapas, and after stressing cultural differences between Indians and ladinos, we must raise the question of social integration. What unites the two groups into something that can be described as a system of social relations? We think that the three main foci of interethnic integration are: a) religious, b) political, and c) economic.

a) The Catholic Church is definitely one of the major integrative factors between Indians and ladinos. This is true in spite of the differences in religious beliefs, practices, and values between the two groups that we have mentioned before. It is not so much, then, the *ethos* of Catholicism as the *formal structure* of the Church which ladinos and Indians share. Ever since the pro-Indian stand of Fray Bartolomé de las Casas (after whom San Cristobal de las Casas was named), the Catholic Church has defended a universalistic and equalitarian position on racial and ethnic issues. Today, posters pleading for "priests of all races to build a universal Church" are displayed at the entrance of most churches in San Cristobal. Though there are no Indian priests, common membership of ladinos and Indians in the Church has undoubtedly been a major integrative influence in Chiapas. Perhaps the most important interethnic religious cement is the complex of ritual kinship known in the Spanish world as *compadrazgo*. Many ladinos are godparents of Indian children and, hence, by extension, *compadres* and *comadres* of the Indian parents of those children. More will be said about interethnic ritual kinship later.

b) The local government in Chiapas is organized on the basis of municipios or counties. The government of San Cristobal, the main municipio in the

TABLE 1. BASIC ZINACANTAN AND SAN CRISTOBAL\* ORIENTATIONS

	Zinacantan	San Cristobal
Religious orientations	<p>More pluralistic view of religion and universe. Belief in both Catholic and pagan deities.</p> <p>Little secularization. Wide adult participation in religion through <i>cargo</i> and curing activities.</p> <p>Multiple soul concepts: a spiritual soul of 13 parts and two types-of animal souls.</p> <p>Need to maintain health and complete possession of souls in the face of an unpredictable supernatural universe and an evil social environment. Bargaining and exchanging with the gods. Curing ceremonies for restoring or keeping harmony with the supernatural forces and maintaining the integrity of the soul.</p>	<p>More dualistic view of religion and universe. Roman Catholicism of the Spanish variant.</p> <p>Considerable secularization. Specialization of religious functions in hands of professional-clergy.</p> <p>Single soul concept.</p> <p>Belief in fore-ordained destiny, changeable only through supplication of God and the saints. Confession and sacraments for restoring personal relationship to God.</p>
Relations to society	<p>Ambivalence, uncertainty and anxiety about relations with others.</p> <p>Rigid and intricate pattern of etiquette functions to reduce anxiety about social relations.</p> <p>Hierarchy of position in ceremony and of age-deference in etiquette patterns, but hierarchization of command is relatively weak. Men are to be influenced and persuaded. Adjustment and harmony are crucial social goals.</p> <p>No social classes. Leveling forces of <i>cargo</i>, witchcraft, and envy prevent stratification.</p>	<p>Relatively little anxiety about relations with others.</p> <p>Etiquette not rigid or complicated and functions more to distinguish social class than reduce anxiety about social interaction.</p> <p>Strong hierarchization. Men to be commanded, dominated, exploited; <i>patrón</i> complex. -</p> <p>Rigid social classes. Wealth and education generally inherited. (However, recent tendency towards open class system.)</p>

Relations to society (cont.)	<p>Open competition avoided. Status gained by engaging in ceremonial or curing activity and spending for ceremonial entertainment.</p> <p>Traditional emphasis. A man's role or office is emphasized rather than his personal qualities.</p>	<p>Open competition encouraged. Status gained by attention-attracting activities, show, wealth, and by being a patrón.</p> <p>Slightly charismatic emphasis. A man's personal qualities are emphasized rather than his role or office.</p>
Relations to family	<p>Competition and conflict within the nuclear family is usually intense. Aggression often physical. Little attempt made to hide family conflict from public knowledge.</p> <p>Both men and women condemned for adultery. Women sexually aggressive.</p> <p>Both castration and ascension elements exist in dream interpretations and folklore.</p>	<p>Intra-familial competition not so violent. Aggression mostly verbal. Close ties between brother-sister, mother-son, and father-daughter. Concern for family honor and desire to avoid public knowledge of family conflict.</p> <p>Women condemned, men condoned for adultery. Women sexually reserved.</p> <p>Ascension complex (the desire to be placed above others, to receive admiration) rather than castration complex predominant.</p>
Work and economic orientations	<p>Routine and manual labor valued.</p> <p>Land valued as a source of subsistence. <i>Milpa</i>-making most worthy activity for men.</p> <p>Play and leisure censured.</p>	<p>Routine and manual labor avoided and devalued.</p> <p>Land valued for ownership as a sign of status but agricultural work considered debasing.</p> <p>Play and leisure valued.</p>
Cultural range in space and time	<p>Local orientation limited to areas in work and trade orbit.</p> <p>Vague and shallow perception of historical past.</p>	<p>National orientation; Mexico City the center of "culture" for upper and middle classes.</p> <p>Strong historical awareness of the Conquest and the 1910-17 revolution.</p>

\* For purposes of comparison with San Luis Jilotepeque, Guatemala, we followed in part Gillin's scheme (1951:121-122). The contrasting view we present is valid only for one culture relative to the other, not by absolute standards or by the standards of a third culture.

Chiapas highlands is, of course, entirely a monopoly of the ladinos. In the rural municipios of the hinterland, Indians have sometimes considerable control over their local affairs, although, in other cases, a ladino secretary (often-times the only literate official) is the real power in the Indian municipios (Aguirre Beltran 1953:119-21). At the local level, then, the governmental structure, insofar as it reflects the urban-rural division, tends to maintain the existing Indian-ladino division.<sup>7</sup> The Federal Government of Mexico, however, has been firmly committed since the Revolution of 1910-17 to an equalitarian ideology. Since 1948 that ideology has been implemented through the action of the Instituto Nacional Indigenista (National Indian Institute, hereafter INI) a federal government organization devoted to the improvement of social and economic conditions among Indian groups in Mexico. The work of INI is directed by anthropologists from its central offices in Mexico City. Branches called "coordinating centers" are located in several of the major Indian areas of Mexico, including one at San Cristobal for the Tzotzil and Tzeltal Indians. By building rural roads, schools, clinics, and introducing co-operative stores and modern agricultural methods, INI is breaking down the isolation of the Indian municipios, improving their living standards, and slowly incorporating them into the cultural life of the Mexican nation. The backbone of the Tzotzil-Tzeltal program is the Indian promoter system. Indians are brought in to the coordinating center in San Cristobal from outlying areas and trained as teachers, nurses, and agricultural experts. Once trained these Indians are called promoters (*promotores*) and are returned to their villages or hamlets to teach what they have learned, to implement the INI improvement programs, and to communicate to INI the Indian response to these new programs. The fact that most progress toward INI objectives has occurred in some of the more remote areas accessible only on foot or horseback has clearly demonstrated the efficacy of the promoter system, for only the promoters and the INI school inspectors have been to these villages. It is significant that INI success in Indian areas immediately surrounding San Cristobal has apparently not been as great as in some of the more distant areas. One reason may be that proximity to ladino society has developed mechanisms of stronger reactive resistance in the nearer Indian cultures. In spite of a much greater effort by INI in these closer areas—including the installation of lighting and public address systems, the building of roads, clinics, a cement basketball court, a ladino-style plaza, and the frequent visits of INI administrative officials from the coordinating center—they continue to maintain greater psychological and cultural resistance than the more remote areas. Some change is occurring, however, even in the resistant communities and in coming years as more funds are allocated to the INI program and as more promoters are trained greater changes will undoubtedly ensue.

c) Economically, San Cristobal and its environs constitute a unit. The Indians produce the food surplus from which San Cristobal lives. San Cristobal with its daily market, on the other hand, is the major distribution and consumption center for agricultural products in the region. The town is also a

major productive and distributive center of manufactured products (such as textiles, metal goods, etc.), a producer of services (hospitals, prisons, barber-shops, etc.), and an important labor market. It is clear that, if Indians and ladinos ceased to have trade relations, neither group could subsist in its present state. It is also true that a majority of the daily contacts between ladinos and Indians involve an exchange of goods and services. Every morning, including Sundays, thousands of Indian men and women stream into San Cristobal carrying heavy loads, mostly of wood and food. These goods are either sold by the Indians themselves in town, or bought from them at the outskirts of town by ladino women who, in turn, resell the goods at a profit in town. These ladino merchants, known as *atajadores* (literally "those who intercept") or *esperadoras* ("the waiting ones") wait in groups for the Indian peasants, descend upon the Indians when they appear, bully them into selling at prices considerably below the market price, and share the profits. This form of trade has been described by Bonilla Dominguez as "purchase by assault" (Bonilla Dominguez 1953:8). When the Indians sell directly to customers in town they either peddle their goods from door to door or sell them on the market place. All regular stands in the market are rented by ladinos, however, so that Indian merchants are left with the less desirable locations. Indians sit and spread their goods on the ground. Bargaining is a part of most transactions. Excepting the Indian food, pottery, and wood merchants, all the trade of San Cristobal is in the hands of ladinos. Indians, therefore, have to buy all their durable and manufactured products from ladino merchants. As customers, Indians are often treated curtly by ladino merchants and cheated on weights and prices. In the words of one informant, "for an Indian, a kilo is 700 grams." The Indians are aware of being cheated, but accept it as an inevitable part of commercial transactions with ladinos (Bonilla Dominguez 1953:9). Diseased meat has been known to be set aside for sale to the Indians (Villa Rojas 1959:3). Conversely, Indian merchants are sometimes bullied by ladino customers into selling at prices lower than those commanded by ladino merchants. In commercial transactions, the term *marchante* is used as a reciprocal term of address by both merchants and customers, whether among ladinos or between ladinos and Indians. Whether as merchant or as customer, however, the Indian is always in the subordinate position when trading with ladinos.

Indians and ladinos also exchange services. In the great majority of cases the Indians are employed by ladinos rather than the other way around. The Indian girls work as servants in ladino households. Indian men do odd jobs in ladino houses, work on highway construction, or are hired as common laborers on coffee plantations. San Cristobal streets are swept by Indian male prisoners in the town jail.<sup>8</sup> The common features of all the work done by Indians are its menial nature and its low rate of pay (between 4 and 8 pesos a day for common laborers; up to 2 pesos a day for a maid, 1 peso = U. S. \$.08). Lower class ladinos do comparable work for comparable pay, however, except for agricultural work which is considered to be strictly Indian work.<sup>9</sup>

In the rural areas, Indian-ladino relations have gone through several stages.

After the Conquest, the Spanish Crown established the *encomienda* system. This feudal system, under which Spanish colonists were granted large land tracts and the right to exact labor and tribute from the Indians, was evolved to reward the Spanish colonists and keep the Indians under the control of the Spanish crown. Later, when the *encomienda* system was abolished, large landowners had a tighter, more absolute control over their Indian labor. Exploitation of Indian labor under conditions of peonage and debt slavery persisted until the Revolution of 1910-17. Until that time, landless Indians known as *baldíos* were forced by need to offer their lifetime services to ladino plantation (*finca*) owners in exchange for a small plot to cultivate for themselves and their families. Following the Revolution, the *baldíos* were emancipated, many of the ladino *fincas* were expropriated, much of the land was returned to the Indians, and labor laws protected agricultural workers. The coffee plantations, which were expanding in Chiapas during this time, resorted to a form of labor contracting opprobriously called the *enganche* ("hooking") system. Under this system, an estimated 15,000 Indians a year are now being recruited on a seasonal basis by *finca* representatives known as *habilitadores*. Indian workers are often recruited when drunk and encouraged to contract debts to the *finca* owners so that they can be kept in a quasi-servile condition. As the coffee *fincas* are located in "hot country" in the lowlands of the state, many Indian men spend several months of each year far from their homes in the highlands. Through continued government and INI intervention, the more glaring abuses of the *enganche* system are slowly being eliminated, however.

One of the few instances where an Indian hires the services of a ladino occurs when an Indian wants a haircut. By custom, Indians enter only some of the poorer barbershops in town, and when they do they are given distinctively inferior service. They are made to sit on a kitchen chair (although the shop has a regular barberchair reserved for ladino customers,) and given a rough five-minute haircut. However, they pay only 1 peso instead of the regular 3 to 4 pesos.

Whether as customers or as merchants, as employees or as employers, then, the Indians are almost invariably in the subordinate role when dealing with ladinos.

Besides the obvious subsistence functions of Indian-ladino economic exchanges, these exchanges make for daily interethnic contacts that otherwise would not take place. Through frequent trips into town, the Indians gradually gain some command (if only rudimentary) of Spanish, and learn about the dominant ladino culture. At the same time, ladinos having most frequent associations with Indians learn words and phrases of Tzotzil (and less often, of Tzeltal, whose speakers live at a greater distance and visit San Cristobal less often) that are useful in bargaining. The various forms of economic exchange between ladinos and Indians account for a large proportion of daily interethnic contacts and are definitely one of the major foci integrating ladino and Indian cultures.

We may now turn to a more systematic characterization of the ladino-



Indian system of ethnic relations. Generally, ethnic relations in the Chiapas highlands are of a type that one of the authors has called elsewhere "paternalistic" (van den Berghe 1958). Ladinos treat Indians condescendingly, but often with a touch of affection. Indians are considered primitive, uncultured, ignorant, unreliable, irresponsible, and childish. They are referred to by ladinos as "*inditos*" (literally, "little Indians") or "*muchachos*" (i.e., children) regardless of age. The titles of Don and Señor are not normally extended to Indians, although in constant use among ladinos. Ladinos invariably address Indians in the familiar "tu" form, the same form in which ladino adults address ladino children up to adolescence. Indians, on the other hand, when addressing ladinos use titles of respect such as Don, Señor, and patrón. Indians are also expected to use the polite "usted" form when speaking to ladinos, but they frequently use the familiar "tu" form, due to their imperfect knowledge of Spanish.

Ladino children can often be observed making fun of Indian adults and playing practical jokes on them in the streets or in the marketplace. We have never observed Indians reacting verbally to these taunts, much less retaliating, though they are often visibly annoyed.

Up until a generation ago, Indians were expected to walk on the streets, the sidewalk being reserved for ladinos, but this custom has now disappeared. However, in crowded places such as the open market, ladinos are constantly seen pushing Indians aside. Until recently, Indians were not allowed to ride on horseback in the Oxchuc area. They also were previously required to fold their arms and bow in a submissive position when speaking to a ladino patrón. While ladino customers on the market are spoken to courteously by ladino shopkeepers, Indians are addressed curtly, though not necessarily in an unfriendly manner. When ladino and Indian customers wait together to be helped, ladinos are generally served first. This rule applies even when the ladino shopkeeper has already started serving an Indian customer.

Formal segregation has only been observed in barbershops as described above, and, even there, the price differential in haircuts may account in part for the custom. However, when a ladino barber was asked why Indians sit on separate chairs, he answered that the Indians were dirty, and that his ladino customers might not like to sit on the same chair where an Indian had just sat. Most Indians are, of course, debarred by distance of residence and poverty from participating in much of the San Cristobal life. Students in the San Cristobal secondary schools are all ladinos, for example, but there exists no formal barrier to the entry of a qualified Indian into those schools.<sup>10</sup> Similarly, few Indians can afford to go to the town's movie theater, but they are not refused admittance, and some do, in fact, go. At the great annual fiesta of the town's patron saint, a number of Indians attend. Many of them wear ladino clothing which makes them indistinguishable from lower class ladinos, except for the language they speak. These "*indios revestidos*" (literally "re-clothed Indians") tend to be onlookers on the fringe of events rather than participants in the merrymaking.<sup>11</sup>

Few Indians worship in the San Cristobal churches, but, when they do,

they sit anywhere they want. In some of the rural churches, however, such as in Tenejapa, Indians and ladinos each have their own saints facing each other on opposite sides of the church, and the religious fiestas of each group take place on different days with either all-ladino or all-Indian participation (Aguirre Beltran 1953:114; Villa Rojas 1959:3).

Whenever Indians and ladinos are seen walking or talking together in town, they are engaged in a commercial transaction, or the Indian is the servant of the ladino (as when an Indian maid accompanies her mistress to the market to carry the baskets). We have never observed purely social contacts of an equal nature between ladinos and Indians, except at the INI. The Indians use the central town square quite freely and sit on benches, but ladinos tend to avoid sitting on the same benches as Indians. Commensality between ladinos and Indians is rare, and we have never observed a case. Indians enter ladino houses quite frequently but as servants or peddlers, not as equals. When an Indian from the countryside stays in town overnight, he sleeps in the hallway of the house of his ladino patrón. The custom is known as "*posada*" and, though it leads to long-term and often amicable relationships between patrón and Indian, the relationship is anything but equalitarian.

Among the most important relationships that cross the ethnic line are the ties of ritual kinship. Anyone familiar with Hispanic culture knows the importance of the *compadrazgo* complex (Foster 1953; Mintz and Wolf 1950; Spicer 1954). At baptism, confirmation, and marriage a person is given one godparent of each sex who is responsible for the person's spiritual development. The godparents and the biological parents of the child term each other "*compadre*" and "*comadre*." In all cases, the biological parents of the child seek out the godparents, never the other way around. Godparents may, in fact, be relatives of the child, but more often than not they are biologically unrelated. A good Catholic, when called upon to serve as godparent, cannot, in conscience, evade the responsibility. The tendency is generally for the parents to seek persons of influence and wealth as godparents of their children so that the number of one's godchildren is always a sure index of social status (Foster 1953). It follows from the above, that ladinos are often sought by Indians as godparents, *but never, or virtually never, the other way around* (van den Berghe and Colby 1961). Many influential ladinos have so many Indian godchildren that they do not know their actual number, which may run up to one hundred or more. Indian parents and ladino godparents call each other *comadre* and *compadre*, but the term, although reciprocal, does not imply equality. The ladinos derive prestige from having Indian godchildren. The Indians, on the other hand, get protection and a measure of economic help from the ladino *compadres* (who may be asked for loans of money and other minor favors).

The complex of ritual kinship, then, makes for lasting and friendly ties between ladinos and Indians without threatening to subvert the subordination-superordination relationship between the two groups. *Compadre* relations across ethnic lines are definitely unidirectional and not equalitarian.<sup>12</sup>

Sexual relations across the ethnic line are relatively frequent. The most frequent form they take is concubinage between Indian girls and ladino men. Indian servant girls, both in town and on the coffee plantations, are reputed to have frequent affairs with their ladino masters (Villa Rojas 1959:3). Intermarriage between ladino men and Indian women is also common, but generally confined to the ladino lower class. Cases of intermarriage between ladino women and Indian men are much rarer and also confined to the ladino lower class. Where it happens, the Indian men are strongly "ladinoized." The town's prostitutes, who are all more or less ladinoized, are visited both by ladinos and by Indian men in ladino clothes.

We have already emphasized that the definition of the various groups in Chiapas is cultural rather than physical. The ladino group is characterized by its possession of the local variant of Spanish culture, not by any "racial" traits. Indeed, while the Indians are genetically homogeneous, the ladinos range from White to Indian in phenotype. If an Indian acquires a fluent knowledge of Spanish, dresses in ladino clothing, and adopts ladino customs, he can "pass" the ethnic line.<sup>13</sup> During his own lifetime his origins may be remembered and he may still be called an Indian, but he will be treated as a ladino for most everyday purposes. His children will definitely be considered to be ladinos. The change of status from Indian to ladino is thus difficult, but not impossible.<sup>14</sup> When an Indian "passes," however, he always enters the ladino class structure at the very bottom. The fact that the great majority of lower class ladinos is phenotypically indistinguishable from Indians is evidence enough that "passing" is frequent.

In no sense, then, can the ladino-Indian system of stratification be described as a caste system, since the ethnic groups are neither closed nor strictly endogamous. Once an Indian has entered the ladino lower class, however, his physical appearance handicaps seriously his ascension in the middle or upper class. This is true in spite of the fact that racial prejudice *as such* is minimal, and that wealth and education are more important than physical traits in determining class status. Rather, the difficulty of *class* mobility has kept the ladino upper and middle class predominantly White or light mestizo, so that the presumption of lower class status is always strong in the presence of an obviously darker person. At a meeting of an upper and upper middle class men's civic club, for example, of the eighteen members present, sixteen looked White, only two appeared somewhat mestizoized, and none was distinctly Indian-looking.

Although downward class mobility within ladino society is possible, downward mobility from ladino to Indian culture is unknown, except by marginally ladinoized Indian servant girls who subsequently return to their village and marry Indians. The ethnic line, then, is open almost exclusively in the "upward" direction.

Ladinos, even the most liberal, accept the postulate of their cultural superiority as self-evident, while at the same time rejecting, for the greater part, any notion of racial superiority. The difference between liberal and preju-

diced ladinos is that the former view the ladinoization of Indians with favor, whereas the latter view it with hostility, or, at best, with ambivalence. Both liberal and prejudiced ladinos, however, view Spanish culture as unquestionably superior (Aguirre Beltran 1953:113). Even the INI has as its official policy the integration of Indian minorities into the Mexican nation via hispanization. The ladino assumption of cultural superiority is, thus, unwittingly supported and reinforced by the official ideology of the present liberal Mexican government.

The Indians seem, for the most part, to have accepted ladino superiority as an inescapable fact of life (Aguirre Beltran 1953:116). Their attitudes towards ladinos are, however, laden with ambivalence. Indian folktales, such as that of Petul—an Indian culture hero who outwits and plays dirty tricks on ladinos—reveal aggressive feelings towards ladinos. One Indian myth attributes the “mean” (“*bravo*”) character of the ladinos to their origin as the offspring of a woman and a dog. Although hostility towards the ladinos is generally repressed, several large-scale rebellions have taken place. Shortly after the Conquest, some Indians rebelled against the Spanish in 1524 and 1526. However, it seems that the presence of the Spaniards acted principally as a catalyst for previously existing rivalries between Indian groups. Some Indian groups fought as allies of the Spanish in these two revolts. A period of enthusiastic culture change, christianization, and hispanization of the Indians followed the early revolts (LaFarge 1940; Beals 1951:228). In 1712, a messianistic rebellion of the Tzeltal groups broke out, and in 1869 the Chamulas attacked and nearly entered San Cristobal. The 1869 rebellion was likewise messianistic in character (Aguirre Beltran 1953:119, 141). On Good Friday, 1868, an Indian youth was crucified so that the Indians would have their own Christ. The cult of the “talking saints” which has enjoyed a recent revival, originated during the Chamula upheaval (Thompson 1954:19–21). Threats of revolts appeared in the early part of the 20th Century, and in 1935 fighting in Cancuc resulted in a substantial death toll for both sides (Aguirre Beltran 1953:117). Although the era of tribal unrest seems to be over, the recent false rumor of a Chamula revolt led to a panic in San Cristobal.

Since the Mexican Revolution, a number of factors have contributed to changing Indian-ladino relations in Chiapas. Land reform has jeopardized the position of many ladino upper class families and labor legislation has emancipated Indian agricultural workers. More recently, INI has introduced a widespread program of sanitation and education among the Indians and has generally protected the Indians against many forms of exploitation and discrimination on the part of the ladinos. The appearance, within the past few years, of Indian-operated stores in the Zinacantan center and in outlying hamlets, and of an INI sponsored cooperative store in Zinacantan, indicates a significant change in economic relations between Indians and ladinos. The recently completed Pan-American Highway and the INI-built rural roads make the region easily accessible to the rest of Mexico, foster the urban development of San Cristobal, and facilitate ladino-Indian contacts. Younger Indian men tend to

be most widely traveled, to speak more Spanish, and to become increasingly ladinoized (van den Berghe and Colby 1961). All these trends combine to undermine the traditional paternalistic type of Indian-ladino relations and to bring about the gradual hispanization of the Indians. San Cristobal, as the largest ladino town in the highlands of Chiapas, is the major focus of these changes.

We may now attempt to summarize the Chiapas ethnic situation. A superordinate minority of ladinos concentrated in San Cristobal lives among a subordinate majority of Indians dispersed in the rural hinterland. As the differences between the groups are cultural rather than racial, Indians can and do become ladinoized, although the process is neither easy nor rapid. Interethnic concubinage is frequent, and intermarriage occurs, though not commonly. Ritual kinship ties growing out of common membership in the Catholic Church are frequent but unequal and unidirectional. The ethnic division of labor is clear-cut and complementary with the Indians as peasants, unskilled laborers, or servants, and the ladinos as merchants, artisans, clerks, and professionals. Ladinos generally treat Indians with the condescending kindness accorded to a backward child. Indians, though latently ambivalent and sometimes hostile to the ladinos, are outwardly accommodated to their subservient status. Rigid rules of etiquette maintain the social distance between Indians and ladinos, but physical distance in the shape of formal segregation is minimal. Contacts between Indians and ladinos are frequent, particularly in the sphere of economic exchanges, but only exceptionally are such contacts of an equalitarian nature. Since post-revolutionary days, governmental action, economic transformations, and improvement in means of communication have combined to remove gradually the handicaps under which Indians have traditionally found themselves.

#### *Ethnic Relations in Chiapas and in Guatemala: a Comparison*

Indian-ladino relationships in Guatemala have been well documented in a number of studies (Adams 1956; Gillin 1947, 1951; Redfield 1956; Tax 1942; Tumin 1952, 1956; Wisdom 1940; Wagley 1949). From these studies and from the present study, many similarities emerge between the ethnic situation in Guatemala and Chiapas. The geographical proximity of the two areas, their close political ties until the early 19th century, and the cultural similarity between ladino and Indian groups in the two regions, all contribute to the resemblance in patterns of ethnic relations on both sides of the Mexican-Guatemalan border.

In both areas, a minority of Spanish-speaking ladinos lives among a majority of non-Spanish speaking Indians belonging to a variety of Maya-speaking groups. On both sides of the border, the line of cleavage between Indians and ladinos is cultural rather than racial and ladinos are strongly mestizoized. Indians are mostly maize-growing peasants and ladinos are concentrated in urban centers in Guatemala as well as in Chiapas. Similar hierarchical relations between ladinos and Indians prevail in the two areas with many re-

semblances in rules of etiquette, division of labor, interethnic compadrazgo, etc. Our earlier discussion of ladino as contrasted to Indian values in Chiapas coincides largely with the contrasting schema given by Gillin (1951:121-22) and with observations made by Tumin (1952:117; 1956:184). Ladinos in both areas look down on manual labor and are stringently stratified into classes, and in both cases interethnic compadrazgo is frequent but unidirectional (Gillin 1951:61; Tumin 1952:131).

Our own findings that ladinos express more social distance toward the Indians than the Indians do towards the ladinos agree with Tumin's findings in San Luis Jilotepeque, Guatemala (1952:133-135, 239-241). Objective differences between Indians and ladinos in dress, language, division of labor, education, type of housing, etc., are likewise very similar in the San Cristobal and the San Luis areas (Tumin 1952:25-37, 71-121; Gillin 1947:339). Patterns of intergroup etiquette such as the ladino avoidance of the title of Señor when talking to an Indian are found in both areas (Gillin 1947:339; Tumin 1952:184-186). Interethnic concubinage between Indian women and ladino men occurs in San Luis as well as in San Cristobal (Gillin 1947:338). In both areas, Indians are resigned to their lower status and do not express overtly their aggression against the ladinos (Gillin 1947:340-43; Tumin 1952:170).

This list of specific similarities across the Mexican-Guatemalan border could be extended. In view of the common or closely related historical and cultural heritage of the two areas, these similarities are not surprising. Indeed, *differences* between the two areas, to which we shall now turn, are more problematical than similarities.

The most apparent difference between the Chiapas situation and the Guatemalan one is the greater rigidity of the ladino-Indian line in Guatemala. Both Gillin (1947, 1951) and Tumin (1952) describe the San Luis ethnic barrier in terms of "castes." Whereas in San Cristobal interethnic marriage occurs and "passing" is relatively frequent, in San Luis acquisition by an Indian of Hispanic culture does not make him a ladino unless he moves to another community where his origins are unknown, and intermarriage between the two groups is not practiced (Tumin 1952:207, 215; Gillin 1947:338; 1951:53). Furthermore, ladinos in San Luis seem to be much more conscious of physical differences between Indians and themselves than the San Cristobal ladinos (Tumin 1952:63-64). Tax (1942) and Redfield (1956), on the other hand, report a less rigid situation more akin to the Chiapas one in their studies in western Guatemala. Tumin suggests that such differences as are reported by the various investigators within Guatemala are the result, not so much of divergences in interpretation, as of actual differences in the local situations. He contends that "caste" divisions are more rigid in eastern Guatemala (where his own community of San Luis Jilotepeque is located) than in western Guatemala (where Redfield and Tax made their studies) (Tumin 1956:174-175).

As the above studies were made in small towns, it is interesting to compare

Quezaltenango, Guatemala's second largest city with a population of 27,700 in 1950, to San Cristobal in Chiapas. The two towns are of comparable size, yet the ethnic situation is quite different. As in many other towns of Guatemala, many Quezaltenango Indians live in town and the Indian group is itself stratified into socio-economic classes. Neither of these conditions prevails in San Cristobal. While most of the Quezaltenango Indians know Spanish, most of them continue to speak their language and the women still wear Indian clothes. Some of these urban Indians are quite well-to-do. Roughly 20 percent of the artisans are Indians and most of the bakeries are owned and operated by Indians. One Indian who recently died is reputed to have had a half million dollars and another owns a large hotel and other business. Indeed, Indians may be seen in almost any occupation from bank clerk to auto mechanic.

Indian students from Quezaltenango attend high schools and universities. One has studied English at a university in the United States and a wealthy Indian sent his sons for training in Germany. An estimated 10 percent of the registered members of the Lincoln Library (established by the U. S. government) are Indians. Before the recent conservative revolution, a number of Quezaltenango Indians held political posts both in Guatemala and abroad.

Wealthy, educated Indians in Quezaltenango are the object of mixed feeling on the part of the ladinos. While ladinos do not address them in the familiar form, and some Indians may even be addressed with the title of Don, there is resentment about their competitive economic position. Indians are sometimes said to be "taking over the town plaza," i.e., the central business district. One informant has heard a noted ladino advocate the machine gun as the best Indian policy, though this was an unusual instance. Although we could determine no cases of segregation, an informant once heard ladinos complain about wealthy Indians sitting in the higher priced section of the movie theater.

Upper class Indians appear quite culture conscious. Recently, an academy of the Maya-Quiché language was formed to study, purify, and preserve Mayan languages and to create a special alphabet for the Quiché language. This Indian group also makes excursions to Indian towns and ruins.

Marriage is a major barrier between upper class Indians and upper class ladinos in Quezaltenango. A ladino will say that a prospective suitor for his daughter is intelligent, educated, and hardworking, but conclude with the remark that his being an Indian prohibits the marriage.

We believe that the striking differences between ethnic relations in San Cristobal and in Quezaltenango derive, in great part, from a difference in the flexibility in the ethnic line. Quezaltenango, like San Luis Jilotepeque and other Guatemalan towns, has a more rigid ethnic line than San Cristobal. Both in San Cristobal and in Quezaltenango Indians settle in town, but in San Cristobal they "pass" into the ladino group upon acquiring ladino culture and language, whereas in Quezaltenango they rarely do so. In Quezaltenango the urban Indians remain a separate group and, though some may have lost

the ability to speak Quiché or Cakchikel, their identity as Indians is maintained, with the women continuing to wear Indian costume. Insofar as Indians rise in education and wealth, the Indian group becomes internally stratified. Prevented as he is from entering the ladino group, the Quezaltenango Indian resorts to the defensive mechanism of preserving his own cultural integrity and glorifying his own culture, much as U. S. Negroes resort to "race pride" (Allport 1954:230). In San Cristobal, economic and educational mobility are accompanied by "passing" into the ladino group and by entering the ladino class structure. Hence, upwardly mobile Indians in the Chiapas situation are continuously drained out of the Indian group, which remains rural and unstratified.

Two complementary hypotheses may be advanced to account for the generally greater rigidity of Guatemalan ethnic relations as compared to the situations in Chiapas and even more so in Mexico as a whole:

1) Until the democratic revolution of 1944, the ladino population of Guatemala held a monopoly of political power. Only since 1944 has the Guatemalan government introduced a program of social reforms and been committed to an ideology of cultural assimilation of the Indians (Gillin 1947:339, 1951:51). Mexico, on the other hand, underwent its great social revolution in 1910-17, and its government has introduced a series of agrarian and social reforms since that date. Since the period of government intervention in favor of the Indians has been longer in Mexico than in Guatemala it can reasonably be assumed to have affected ethnic relations in Chiapas more profoundly than in Guatemala and in the direction of greater flexibility.

2) In Guatemala, the Indians constitute a majority (53.5 percent) of the total population.<sup>15</sup> In Mexico as a whole, on the other hand, the Indian population consists of a number of small minority groups relegated to the most isolated parts of the country. Even in regions where the ladinos are the minority, as in San Cristobal, the ladino middle and upper classes identify with the national community and, like most Mexicans, view the country's population as consisting basically of a culturally homogeneous group of hispanicized mestizos. In Guatemala, the near ubiquity of the Indians and the high visibility of cultural and linguistic differences leads, on the part of the ladinos, to a much greater consciousness of ethnic differences, and to a dichotomous view of the national community.

The dimension of flexibility of ethnic line has some important consequences in the development of ethnic relations through time. One of the authors has presented elsewhere a dichotomous ideal-type model of race and ethnic relations (van den Berghe 1958). The paternalistic type of group relations is typically found in pre-industrial societies with little social or geographical mobility, a clear-cut division of labor along group lines, and a wide gap in wealth, education, and/or culture between dominant and subjugated groups. Intergroup relations are of the master-servant variety with benevolent despotism on the part of the dominant group and submissive accommodation on



the part of the subjugated group. Such relations are generally intimate, but inequality of status is strictly enforced. Urbanization and industrialization undermine this paternalistic system which then tends to evolve towards the competitive type of group relations *if the racial or ethnic line remains rigid*. The economic and social rise of the subjugated group is then perceived as a threat by the dominant group. The lower group ceases to be accommodated and is viewed by the dominant group as "uppity," aggressive, pushy, and dangerous rather than as childish, immature, inferior but lovable so long as it remains "in its place."<sup>16</sup> Whenever the ethnic or racial line becomes fluid, however, the competitive stage may be by-passed to the extent that group distinctions become obliterated through acculturation and miscegenation.

To return to our Chiapas-Guatemala comparison, we view both regions as having inherited from colonial times a paternalistic system of ladino-Indian relations. In San Cristobal and in much of Guatemala the present system is still predominantly paternalistic but rapidly changing. The direction of future change in both areas is going to be determined in part by the degree of ethnic-line rigidity prevailing in each local setting. In Chiapas, the relative flexibility of the line (as indexed by the amount of passing, and the assimilative ideology of the upper and middle class ladinos towards the Indians) is reflected in the virtual absence of competitive elements, with most exceptions among the lower classes. As Indians become acculturated and rise in education and wealth they enter the urban ladino class structure. Although the total assimilation of the Indians in Chiapas is far from completed, the long-range prospect for an integrated, mestizo-hispanic society in Chiapas is good. For Mexico as a whole, this assimilative process is already very advanced.

In contrast, the Quezaltenango situation with its competitive elements shows the direction of change from a rural, folk, paternalistic system of ethnic relations to an urban one where the ethnic line remains rigid. Of course, the virtual absence in Guatemala of racism on North American or South African lines makes it very unlikely that a virulent form of competitive prejudice will ever develop in Guatemala. Cultural pluralism accompanied by invidious group distinctions, however, may be expected to persist longer in Guatemala than in Mexico.

#### NOTES

<sup>1</sup> Among the many people who helped us at various stages of our study we should like to mention G. W. Allport and E. Z. Vogt of Harvard University, J. de la Fuente, A. Villa Rojas and F. Montes Sanchez of the Instituto Nacional Indigenista, L. Velasco Robles, P. Moscozo Pastrana, J. Baroco, F. Blom, J. M. Duran Aldana and our many Zinacantan and San Cristobal informants who were at all times most kind and helpful. This study is part of the Mexican Cultural Change Project supported by the National Institute of Mental Health and directed by Professor E. Z. Vogt. The present paper is mostly descriptive and qualitative. The quantitative results of our questionnaire and interview studies are reported elsewhere (van den Berghe and Colby 1961).

<sup>2</sup> Originally the word ladino was used to refer to the Indians who knew *the* language, i.e., Spanish. By extension, ladino came to be applied to all persons of Spanish culture as opposed to

the Indians. Ladino also means literally "cunning" or "crafty." The origin of the term is thus clearly cultural, not racial (Adams 1956:18).

<sup>3</sup> The 1950 Census lists 110,233 persons in the 15 Indian *municipios*, but this number, even then, was probably an under-estimate (Villa Rojas 1959:4).

<sup>4</sup> There is some objective economic basis for placing the Zinacantecos on top of the various Indian groups. On the whole, Zinacantecos are better off than the other groups. Zinacantecos often hire other Indians, principally Chamulas, to work for them on their corn fields. Chamulas, on the other hand, seldom, if ever, hire Zinacantecos.

<sup>5</sup> Tumin emphasizes this distinction between ladino and Indian cultures in Guatemala, and speaks of the asymmetry of the two groups' value systems (Tumin 1956:185, 188).

<sup>6</sup> In a semi-urban hamlet on the outskirts of San Cristobal, all Indian inhabitants over 15 were common laborers or domestic servants in ladino households (Bonilla Dominguez 1953:6).

<sup>7</sup> For an analysis of differences in Indian and Mexican justice in a Chiapas Tzeltal community see Metzger (1960).

<sup>8</sup> At the time of our visit the men's jail had 53 inmates, of whom 49 were Indians. The women's prison had 5 inmates, of whom 3 were Indians. No segregation existed in the two jails. Until recently, the San Cristobal police arrested drunken Indians and detained them for one or more days in jail so as to furnish the town with a constant supply of street-sweepers. Even regular courts discriminate against the Indians according to Aguirre Beltran (1953:108,110).

<sup>9</sup> Recently, the municipality of San Cristobal hired a Chamula Indian as policeman. We heard two ladinos express astonishment at his getting such a "good" job. On the other hand, one Zinacanteco informant implied that being a policeman for the ladinos would be beneath his dignity.

<sup>10</sup> However, up until recently, there were separate schools for ladinos and Indians in some of the rural *municipios* where both ethnic groups were being represented (Villa Rojas 1959:3). Recently, Indian youths being trained by INI for future teaching of children in their home areas have entered two of the previously all-ladino schools in San Cristobal.

<sup>11</sup> Up until a few years ago, Indians were debarred from the merry-go-round on the fiesta grounds, but this is no longer the case, due to the action of INI.

<sup>12</sup> On this point we disagree with Tumin who states in his San Luis study that ritual kinship between ladinos and Indians makes for more equalitarian relations between the two groups. Tumin's own description of a baptismal fiesta for the ladino godparents of an Indian child is clear evidence of inequality in the relationship (Tumin 1952:127, 131-32). We are more in agreement with Gillin's interpretation of interethnic *compadrazgo* (1951:61).

<sup>13</sup> In some areas more distant from San Cristobal, such as Oxchuc, Indians wear ladino clothing at all times, thus clothing is not always a distinguishing characteristic.

<sup>14</sup> Minor variations in the degree of ladino receptivity to changing Indians exist in smaller Chiapas communities. For brief descriptions of some of these see de la Fuente (ms.), and for a detailed account of Indian change in a Huastecan community see Miller (1960). An excellent comparative description of change in three Indian areas of Mexico, including Chiapas, appears in de la Fuente (1958).

<sup>15</sup> Of course, the high proportion of Indians in Guatemala can be considered both a cause and an effect of the rigidity of the ethnic line.

<sup>16</sup> The paternalistic-competitive distinction overlaps, of course, with Redfield's folk-urban continuum and Tönnies *Gemeinschaft-Gesellschaft* types. Examples of paternalistic situations are the ante-bellum Southern United States, the plantation regimes of the West Indies, Latin America, and Brazil, and most colonial regimes in their early phase. The modern United States and South African situations and anti-Semitism in much of the Western World are examples of competitive group relations.

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## Experimental Archeology<sup>1</sup>

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### INTRODUCTION

THE term experiment appears in a number of archeological contexts. Generally it is used in connection with either field or analytic methods. In both categories it most often means a trial; a test undertaken for the purpose of evaluating a new method. The appendix title, "Experimental Techniques," in Atkinson's *Field Archaeology*, for example, refers to new field methods which were not commonly in use in 1946, the date of publication, but which were undergoing trial. Willey's (1953:1) study of settlement patterns in the Viru Valley, Peru, termed an experiment, involves testing both a field and analytic method and Rouse's (1939:7, 9) *Prehistory in Haiti*, also called an experiment, is a test of a proposed analytic method.

Something analogous to the thought or imaginative experiment (Benjamin 1936:257) is often used preparatory to field work. Thus Thompson's (1954) excavation of a Roman aqueduct is guided by entertaining and manipulating mental images of the course the aqueduct reasonably could have taken. Less commonly, archeologists perform comparative experiments (Cox 1958:4) in field methods. In "Observations on the Efficiency of Shovel Archaeology," for example, Meighan (1950) tries two field methods with the end of evaluating their efficiency relative to each other. Experiments have also played a role in evaluating whether the shape of certain objects resulted from human or natural agencies. Experiments in this category are most often associated with the enigmatic eoliths of the Old World (for example, Barnes 1939), but similar experiments relevant to New World problems have also been performed (for example, Harner 1956).

Another category of experiments entails operations in which matter is shaped, or matter is shaped and used, in a manner simulative of the past. These experiments, which I call imitative experiments, differ significantly from all the above. The aim of imitative experiments is testing beliefs about past cultural behavior. If archeology is taken to be the study of past cultural behavior, the imitative experiment is the keystone of experimental archeology. The present study is concerned solely with the imitative experiment.

The importance of the imitative experiment is limited to the kinds of problems in which they can be executed. The fact that these problems, dealing mostly with subsistence and technology, cover a relatively narrow range in the total cultural spectrum, does not diminish their utility, for the bulk of archeological data consists of evidence relevant to these areas. The importance of the imitative experiment, therefore, is best judged in terms of its contribution

to the solution of those kinds of problems for which there are archeological data and which, because the data exist, are most often handled by the archeologist. The importance of the imitative experiment might be demonstrated by example.

Legitimate archeological evidence for the practice of agriculture consists principally of either tools believed to have been used in agriculture or botanical remains of domesticated plants. Due to differential preservation, the latter seldom survive, the former are often preserved. In 1934 Garrod reported the recovery of "sickles" from a cave in Mount Carmel. The find excited interest because the "sickles," if they could be interpreted as agricultural tools, would provide evidence for one of the earliest introductions of agriculture in the Near East. The question arose as to whether the luster on the flint edges of the "sickles" was the result of cutting wood, bone, or grass. If it could be demonstrated that the type of luster evident on the "sickles" could be the result of cutting grass, and could not be the result of cutting bone or wood, then the practice of agriculture could be inferred. Previous to Garrod's report, Spurrell (1892), Vayson (1919), and Curwen (1930) performed experiments with parts or replicas of "sickles" or "sickle-like" objects. In these experiments various materials were cut in order to test the belief that different materials would produce different kinds of luster and, hence, the use to which the tool was put could be inferred from the luster. The results of these experiments were inconclusive. After Garrod's report, Curwen (1935) refined the experiments by more closely simulating the amount and kind of wear to which "sickles" would be subject. The results of this imitative experiment indicated that bone, wood, and grass produce distinguishable differences in luster. The type of luster produced by grass was similar to the luster on the sickles from Mount Carmel.

The performance of imitative experiments is not a recent phenomenon in archeological research. Fashions in every discipline change: imitative experiments, once in vogue, are now seldom performed by professional archeologists. At the turn of the last century, which saw archeology "come of age" (Daniel 1952:122), imitative experiments were being performed with regularity. During this era imitative experiments were being performed by, for example, Nilsson (1868) in Denmark, Pfeiffer (1912) in Germany, Evans (1897) in England, Lartet in France (Lubbock 1878:561), Heierli in Switzerland (McGuire 1894:724), and by Cushing (1894), Sellers (1886), and McGuire (1891; 1892; 1893; 1894) in the United States.

In spite of the fact that numerous leading prehistorians (Childe 1956:171; Clark 1953:353; Leakey 1953:30; Movius 1953:165) have dwelt on the potential value of the imitative experiment, this potential has never been realized. The imitative experiment has failed to receive general acceptance because the evaluation of the procedures and results of such experiments are ambiguous. This ambiguity can be traced in part to the fact that the locus of the imitative experiment, and the theory and logic involved in executing imitative experiments, are unclear.

The intent of the present study is not historical, nor is it intended to be a

review of imitative experiments already performed. Although rooted in the past, the purpose of the present work is future directed: it is hoped that the study of the nature of the imitative experiment will provide a sound basis for future experimentation.

This study is the first formal examination of any of the tests which are used by the archeologist to transform a belief about what happened in the past into an inference. In future studies I will consider other aspects of the logic of archeological interpretation.

#### THE LOCUS

The imitative experiment can be used by the archeologist to transform a belief about what happened in the past into an inference. The execution of an imitative experiment involves simulating in the present time that which is believed to have happened in the past in order to test the reasonableness of that belief. Its utility is that of a testing mechanism; its locus is within the inferential process. A formal consideration of the inferential process, and an example of it as it applies to archeology, will serve to identify the locus of the imitative experiment with precision.

According to Thompson (1958), the inferential process can be viewed as a four-step sequence: 1) the recognition of the indicative aspects of the data; 2) the formulation of an indicated conclusion; 3) the introduction of probative data; and 4) the formulation of a probable inference. The sequence is illustrated in the following example in which Thompson (1958:61) demonstrates the formulation of an inference about the probable use of the category "small ceramic bowl" from a Mesoamerican collection. A group of bowls have already been separated from larger bowls on the basis of the idea that the size distinction is relevant. It is important to note that, according to Thompson (1958:148), the probative data brought to bear on the indicated conclusion (working hypothesis) serves to test the original basis of the typological classification "small bowl," as well as the use of the small bowls. Thompson (1958:148) notes that although the working hypothesis has no alternative in the first stages of the inferential sequence, other hypotheses may develop with the introduction of tests.

*Indicative data:* Small bowl.

*Indicated conclusion:* The small bowl was used as a food dish.

*Probative data:* These vessels have a very small size and capacity.

They have an open and unrestricted orifice.

The form is almost identical to the half gourd which is extensively used for eating and drinking throughout tropical America.

Similar vessels are used for eating and drinking in other parts of contemporary Mesoamerica.

*Probable inference:* The small bowl was used as a food dish.

Consider the probative data. In testing the indicated conclusion or working hypothesis, three orders of evidence are introduced: 1) formal properties—

size and capacity of the vessels are viewed as limiting the range of its possible uses; 2) analogy—similar bowls are used as food dishes in a contemporary situation in the same geographic area; 3) skeuomorphism—the bowl resembles similar nonceramic objects which are used as food dishes. Generally, testing a hypothesis in archeology is a matter of analogy either to a specific situation which either is, or is believed to be, a direct continuance of an archeological situation, or by analogy to a more general situation which, although not directly analogous, is relevant. Other testing mechanisms, such as skeuomorphism and limitations imposed by formal properties, as illustrated in the above example, can also be used.

At least one other testing mechanism, not utilized above, can be of value. An experiment can be performed with one representative small bowl in the category small bowl to test whether the items in question could have been used in the manner suggested by the indicated conclusion. If Thompson had done this, he would have performed an imitative experiment.

Consider the following example in which an imitative experiment was performed. The example has been organized in the procedure outlined by Thompson, although originally it was not presented in this manner. The example is primarily meant to illustrate the locus of the imitative experiment within the inferential process.

(1) At Wupatki National Monument, Schroeder (1944:329-30) recovered a reconstructable jar embedded in an upright position in the ground. The vessel (dated A.D. 1075-1275) was situated 72 cm. below and 4 cm. in front of the ledge of a sandstone outcrop. Two natural depressions on the surface of the outcrop converged to a point above the mouth of the jar. A slab house and two cave shelters were within 20 meters of the jar.

(2) On the basis of the spatial arrangement and the desert environment at Wupatki, Schroeder reasoned that the purpose of the vessel was the collection of water.

(3) The fragmented jar was recovered and a container was substituted in its place. During a rainfall of .23 inches no water ran "out of the two depressions above the vessel on the outcrop." Hence, no water collected in the container.

(4) Schroeder interpreted the results of the experiment as follows: "a greater amount of rainfall is necessary (estimated .50 inch) before this method of collection would function. Rainfall of such magnitude occurs only in the late summer in this area and this would imply a seasonal method."

The testing of a belief about what is proposed to have happened in the past is the crucial step in the sequence which leads from the recognition of relevance to inference. This is so because an inference is only as convincing as the positive results of the tests which can be brought to bear on the hypothesis. The failure of a hypothesis to pass examination can lead to seeking alternative hypotheses, or can, as in the above example, lead to an important modification of the hypothesis.



## FIVE CASES

Published accounts of imitative experiments range from the simple statements that experiments were performed (Fichardt 1957:53) to detailed reports (Steensberg 1943). Some experiments have been elaborate, taking years to complete (Steggerda 1941), others less involved have been executed in a few hours (Quimby 1949). Many of the most often cited experiments have been executed by nonprofessional archeologists (Cabrol and Coutier 1932; Mewhinney 1957; Pond 1930).

From this literature, five cases have been selected for presentation. The cases provide material for the examination of the theory and logic of the imitative experiment. The principal criterion of selection of the cases is the degree to which they mirror the theory and logic, never explicitly stated, which provides the conceptual frame within which the experiments are performed. Although an attempt has been made to represent several archeological problems and to detail the results of the experiments, these specific aspects of particular experiments are secondary in the present study. Complex experiments, for example the voyage of the *Kon-Tiki* (Heyerdahl 1950) and Israel's current experiments in prehistoric agriculture in the Negev (Evenari, Shanan, Tadmor, and Aharoni 1961), are not included in the cases because of the difficulties of concise description. However, the principles involved in more complex imitative experiments are no different than those of the comparatively simpler experiments detailed below.

*Case 1. Cave painting media* (Johnson 1957:98-101).

The people or peoples who painted animals and humans on cave walls in South Africa are unknown. The time that the paintings were executed is equally uncertain.

Using as models the cave paintings in the Clanwilliam area, South Africa, Johnson attempted to redraw the figures, varying the medium with each successive attempt.

First, the chemical composition, natural sources (animal, plant, and mineral), and present availability of pigment in the area of the caves were studied. Three sources of pigment, red ochre, bone, and lead oxide were chosen. Each is capable of producing a color present in the paintings. Of the three, only lead oxide does not occur in the immediate area.

The natural sources (plant and animal) and present availability of media in the cave area were then considered. As a result of this analysis, and after consideration of the suggestions of previous literature, eight media, wax resin, marrow fat, mutton fat, hyrax urine, plant juices, bile, honey, and tempera were selected. All are now available at the site.

Each of the media was then independently combined with the pigment and the eight resultant paints were applied to the cave wall using a quill and a bristle brush. Three principal criteria were used to evaluate the success of each medium: 1) the ability of the author to reproduce the fine lines found in

the painting; 2) the amount of the medium needed to reproduce a figure; and 3) the retentive ability of the paint on the cave wall. Two media failed in all three criteria, three failed in two criteria, and two failed on a single criterion. Only tempera was successful in all aspects. The author evaluates the result of the experiment with the statement: "I find it difficult not to be dogmatic about its (tempera) being *the* method." The author notes that since an ostrich and a fat-tailed sheep are depicted in cave paintings in the Clanwilliam area, a source of tempera (ostrich eggs) and a source of mutton fat was available at the time the paintings were executed.

*Case 2. The manufacture of "charmstones"* (Treganza and Valdivia 1955:19-29).

The designation "charmstone" is used in the terminology of California archeology with reference to ground stone plummet-shaped objects which vary in length from 2 to 10 inches. Although the label implies the use of the object for magical ends, tests of that implication by analogy to specific ethnographic situations are inconclusive. The following investigation is concerned with their manufacture.

In an investigation reported by Treganza and Valdivia charmstones were made by Valdivia. Valdivia's technique of manufacture was by pecking and grinding; that is, the stone was worked in the following sequence: percussion fracturing, pecking, crumbling, and abrading.

The purpose of the investigation was "not simply to demonstrate that it was possible to recreate forms, but to experiment with various stone materials and tools known to have been utilized by Indians in the manufacture of pecked and ground artifacts." Specifically, the authors desired to learn "how various rock materials responded to different shaping tools, to understand perfection and latitude in the use of tools, the time factor involved in various stages of manufacture, and the casualty rate in production."

A total of eight tools were used in manufacturing the charmstones. The choice of some was based on tools recovered in association with artifacts in different stages of completion; for others, which were made by Valdivia, the criterion is not given. Presumably examples of them, with the exception of the bow-drill, can be found in archeological specimens recovered in California.

Two aspects of the materials from which the charmstones were to be manufactured were considered before the materials were selected: 1) the type of stone (e.g., steatite); and 2) the size and shape of the stone. The choice of type of stone was based on an examination of a series of authentic charmstones. Size and shape "approximated . . . the desired form of charmstones . . . on the assumption that the makers of the charmstones would . . . also have been interested in reducing the labor of pecking and grinding." Lithic materials used were sandstone of varying compactness and hardness and steatite.

The first three attempts at manufacturing charmstones failed. Two of the failures were due to heavy blows and one to "faulty material" from which the charmstone was to be made. Seven subsequent attempts were successful. The data for each of the seven successful specimens include: 1) the type of stone

and its properties (compactness, hardness, etc.); 2) the tools used in manufacturing; 3) the technique of manufacture; 4) the time for pecking (percussion fracturing, pecking, crumbling); 5) the time for grinding and smoothing; and, 6) the total time. Drilling time is included in the data on two specimens in which a hole was drilled.

Blackwood's (1950) observations on the manufacture of ground stone objects by the Kukukuku of New Guinea are then quoted at length. It is noted that since Valdivia's work was conceived independently of Blackwood's report, the parallels between Valdivia's work and the work of the Kukukuku are of interest. McGuire's (1891) experiments in making ground stone objects are also introduced by means of extensive quotation. The choice of quotations leaves the reader with the impression that there are certain similarities between the quoted observational and experimental data and Valdivia's work.

*Case 3. The notched scapula and ribs* (Morris and Burgh 1954:61-63).

The scapulae or ribs of large mammals, showing considerable wear along a jagged or notched edge or edges, have been recovered from archeological sites in North America. Tools meeting this description occur in widely diverse ecological and cultural areas and at different time levels.

Huscher and Huscher (1943:37-38) recovered such tools in excavations at Mesa County, Colorado. Temporally, the tools may be Basketmaker. The jagged edge, according to the authors, originates in the manufacturing process. That is, the bone is deliberately broken in order to secure a working edge. After considering possible uses of the tools, which had been suggested in earlier literature (e.g., arrow-shaft smoothers, seed beaters), the authors infer that they were used as hide-scrapers. Their decision is based on the hunting emphasis at the sites they investigated, on analogy to "similar" tools which occur in Northwest Coast archeological sites, and on analogy to "similar" tools which are used for hide scraping by contemporary peoples of the Northwest Coast.

Morris and Burgh (1954) recovered notched scapulae and ribs from Basketmaker sites near Durango, Colorado. Taking issue with Huscher and Huscher, they contend that the tools were used in a different manner. Their experiment can be divided into two parts.

In the first part of the experiment a deer hide was stretched over a pole and first an unnotched deer rib, and then a notched deer rib, were used to remove tallow and hair from the hide. As a result of the scraping process, the unnotched deer rib was worn uniformly and no notches formed. The notched deer rib was less effective and the prongs between the notches broke. The authors interpret the results of the experiment as follows: "This suggestion (that the tool could have been used as a hide-scrapers) we have disproved, at least to our own satisfaction."

The second part of the experiment proceeded as follows:

. . . production of cordage was one of the major industries of any Basket Maker community and the principal source of fiber was leaves of the yucca plant. Therefore, we determined to ascertain if the notched tools would be serviceable in separating the fiber from the leaves. The yucca available was of the narrow-leafed species native around Boulder, Colorado. . . .

The first step of the procedure that we eventually adopted was to strip off the edges of the leaf, which are more bark than fiber. . . . Next, the leaf . . . was slowly drawn across a peeled pole about 12 cm. in diameter and hammered with a smooth cobblestone until the bark was well macerated and the fibers considerably beaten apart. A rib from the North Shelter . . . was used to remove the pulp. . . . in less time than was anticipated, the strand of fibers was freed from the coarser bark and pulp. After about two hours of use, here and there the edge of the rib began to break through into cancellous tissue. Presumably the nicks so produced would have deepened into notches had we seen fit to continue the experiment indefinitely.

For further cleaning of the fiber, we gathered the increments provided by individual leaves into hanks around 2 cm. in diameter, immersed them, and repeated the scraping process. Both a straight edged and a deeply notched rib were tried on the hanks. Each was effective, but the one with the notches more so than the smooth. . . .

Having demonstrated that they are wholly effective for the purpose, we do not hesitate to identify notched scapulas and ribs as the tools with which the Durango people prepared yucca fiber for the making of cordage.

Morris and Burgh find "confirmation" of their interpretation of the results of the second part of the experiment in yucca fibers and sap found imbedded in a notched deer rib recovered at the site. They also cite as an "interesting parallel" an ethnographic example (Pima) in which a deer's scapula is used to separate pulp from fiber of a maguey plant.

*Case 4. The arrow-shaft straightener (Cosner 1951:147-48).*

A tool of common occurrence, especially in the later archeological periods of the western United States, is the arrow-shaft straightener. The distinguishing characteristics of the tool are the relative hardness of the stone from which it is made and a groove or number of parallel grooves which run the total length (usually from 2 to 6 inches) of the stone. It has been distinguished from a similarly grooved tool, the arrow-shaft smoother, on the basis of the hardness of stone; the smoother is generally fashioned from sandstone and presumably served as an abrader. The presumed use of the straightener, as its name implies, was straightening wood in order to make it usable for arrow shafts. Ethnographic analogies support the distinction between straightener and smoother.

Cosner (1951) had straightened wood to be used as arrow shafts without using a tool and had observed Pima Indians straightening arrow weed to be used as arrow shafts, also without the aid of tools. He was also familiar with reed cane shafts recovered with a child burial in the Tonto cliff dwellings at Tonto National Monument, Arizona. "Almost convinced . . ." that the tools designated as ". . . 'arrow shaft tools' actually had some entirely different function," he undertook a series of four tests.

The materials used in the tests consisted of: 1) a shale, 3-grooved, 2-inch, arrow-shaft-straightener recovered from a Pinto Creek site; 2) samples of arrow weed; 3) samples of reed cane; 4) water; and 5) a mesquite coal fire.

The first and second of the four tests were performed with arrow weed, the latter two with reed cane.

Tests 1 and 2 (arrow weed):

1. Four samples were successfully straightened into shafts by heating over mesquite coals and bending by hand. No tool was used.

2. The arrow-shaft straightener was heated and an attempt was made to straighten one sample with the aid of the heated tool. Presumably, the sample was passed through the grooves. No further samples were used because: "The first one absorbed so much heat that the shaft had to be heated twice before it was affected much."

Tests 3 and 4 (reed cane):

3. Four samples were "dampened and warmed, and an attempt was made to straighten them by hand." No tool was used. "All four were failures."
4. The arrow-shaft straightener was heated and the reed cane samples were dipped into water. Four samples were successfully straightened by passing them through the grooves of the heated tool. Cosner notes that, in addition to successful straightening, the diameter of the samples could be reduced at any point and the joints of the reed could be made even with the body of the cane.

Cosner interprets the results of the four tests in the following manner:

That the implement in question is really an arrow straightener . . . seems plain to me now. . . . This stone not only proved to be a good way to form cane; it is the only way I know of.

I am of the firm opinion that the stone is used only for the straightening of cane shafts. For solid shafts such as arrow weed it would not be necessary nor indicated.

*Case 5. Copper smelting in the Old World (Coghlan 1940:57-65).*

There exists evidence which indicates that copper was one of the first of the metals altered by man in shaping objects for his own needs. In nature, copper occurs in a virgin state and in the form of sulphide and oxide ores. Virgin copper may be directly shaped into small objects such as awls and pins by cold-hammering, annealing and hammering, or by melting and casting. In order to utilize the copper in copper-bearing ores, the ores must first be smelted. The knowledge of smelting was important not only because it released a potential source of copper but because the knowledge is necessary to produce copper alloys, such as bronze.

The knowledge that copper can be obtained from ores was probably obtained accidentally. The most widely accepted view of what that accident was, is that copper was discovered in the remains of a campfire into which some copper-bearing ore had been introduced unintentionally. Those who hold this view suggest that the first metallurgic hearths were campfires or simple "hole-in-the-ground" furnaces. Experiments by Coghlan bear directly on the problem of the origin of copper smelting in the Old World.

Coghlan constructed a "hole-in-the-ground" furnace. It consisted of a one foot circular hole surrounded by a stone wall three feet in diameter. A charcoal cone was constructed in the furnace and two layers of small pieces of malachite, separated from each other by charcoal, were imbedded in the cone. The charcoal was ignited and after it became hot more charcoal was added and the fire was permitted to burn for several hours. The experiment was performed on a windy day to permit as much natural draught as possible. The result was negative, i.e., no "useful metal" was obtained. The process was repeated with cuprite and results were again negative. Both malachite and cuprite are

oxide ores. Sulphide ores were not considered necessary to experiment with as they "generally occur at a much greater depth than the oxidized ores, and so would not have been the first ores used by early man; also technically they are much more difficult to deal with." The experiments failed, according to Coghlan, because the "necessary reducing atmosphere" could not be obtained. Coghlan concludes:

Since the failure of the experiments indicated that the camp-fire, or "hole-in-the-ground" fire was very unlikely to have been the first metallurgical hearth, the only suitable remaining source of heat would seem to have been the pottery kiln or furnace. As the use of pottery certainly predated the discovery of smelted copper by a considerable time, it is quite possible that the closed furnace, or possibly some form of reverberatory furnace, may have been in use. . . . If a copper carbonate ore such as malachite were introduced into a pottery kiln, the thermal conditions would be favorable for the reduction of the ore to take place; to test this, the following simple experiment was carried out.

Coghlan placed a "small lump" of malachite on a ceramic dish and inverted a ceramic pot over the dish. The arrangement was based on early pottery kilns which consisted of a dome of brick or burnt clay around which a fire was built. The "miniature kiln" containing the malachite was placed on hot charcoal and ashes and then imbedded in a cone of burning charcoal. After several hours the "kiln" was removed and its contents were examined. A spongy copper was produced. The experiment was repeated, but instead of using a malachite lump, the malachite was first "ground to a small size." The result was a compact copper loaf. Coghlan then states:

The conclusion to be drawn from these experiments would seem to be that if a piece of malachite, or ground malachite, were left accidentally in the baking chamber of a divided, or reverberatory pottery kiln, it would become reduced, and since the baking chamber would not contain any fuel, the resulting copper would be easily noticed.

The question whether malachite could have gotten into a pottery kiln of course depends on whether malachite was ever used for the decoration or painting of early pottery.

Coghlan argues that there are two possible ways that malachite could have been used in pottery decorations: 1) as a slip or paint; and 2) as a constituent in a glaze. With regard to the first possibility Coghlan performed an experiment, the details of which are not given, which indicates that malachite, when used as a slip or paint, yields a "fine" black surface. An analysis of early Egyptian glazes shows 18.5 percent copper oxide. Since there is evidence that glazing was introduced into Egypt in the First Dynasty from an outside source, glazing would be earlier than the First Dynasty. Coghlan concludes:

To sum up, it seems to the writer that the accidental reduction of a piece of copper ore, most likely the ore malachite, which led to the discovery of the knowledge of how to smelt copper from its ores, was probably made in a pottery kiln . . . .

#### GENERAL PROPOSITIONS

The archeologist, in performing an imitative experiment, seeks to test the reasonableness of such statements as "the medium used in these paintings was plant juices" (Case 1). Singular statements of this type, generally referred to as ideographic propositions, are thought to be the aim of historical, as distinguished from theoretical, endeavors (Nagel 1952:161-62). An examination

of the above statement will show that propositions of a general character are also involved.

The statement "the medium used in these paintings was plant juices" is meant to describe the medium which may have been used by any member of a community and not by any specific individual within that community. Thus, the statement, while being special to a context (particular spatial area and time span), is not unique within that context. (Many individuals may have used plant juice as a medium on many different paintings.) The statement is, therefore, an implicit description of cultural behavior, where culture is taken to mean "... capabilities and habits acquired by man as a member of society" (Tylor 1871:1). The statement can be accepted as a legitimate proposition only if one is prepared to assume that: 1) cultural behavior is "... something more than an endless series of haphazard items ..." (Kroeber 1948: 336); and 2) cultural behavior (e.g., the use of plant juices) can be inferred from its material results (i.e., in paintings).

It would be a mistake to believe that, because the archeologist performing an imitative experiment is primarily concerned with cultural behavior which is localized in space-time and in a culture, he does not implicitly employ general propositions. These propositions, generally considered within the domain of theory, are sometimes drawn from other fields. In archeology general propositions function, as they do in history (Danto 1956:20; Gardiner 1952: 45), as broad working hypotheses.

The first of two general propositions which serve in the performance of imitative experiments is drawn from general anthropology: "*All cultural behavior is patterned*" (italics mine; Sapir 1927:118). The proposition is not novel: in fact, it is difficult to imagine how any effective inquiry could begin without the supposition that some kind of order exists in the subject matter of inquiry.

The particular kind of order subsumed under the pattern concept, key in the above proposition, has often been obscured by its ubiquity. The term has been used to umbrella a multitude of phenomena, at different levels of generality, investigated in diverse ways. For example, the phenomena embraced under the concept are as varied as society (Wissler 1923), plow agriculture (Kroeber 1948:313), a cultural personality syndrome (Benedict 1950), polite and impolite breathing (Sapir 1927:117), and taking or not taking a second mate (Kluckhohn 1941:119). Attempts at ordering the concept of pattern have sometimes compounded ambiguity by coining overlapping subsidiary concepts. Thus, Linton's "real patterns" (1945:45), Sapir's "action patterns" (1934:411), and Kluckhohn's "behavior patterns" (1941:117) refer essentially to the same kind of observed activity, on the same level of generality, investigated in similar ways. In spite of these and other difficulties (see, for example, Weakland 1951; Cohen 1948), there appears to be general agreement with regard to the ideas implied in the pattern concept.

In general, the kind of order which the term "pattern" connotes is that of direction, tendency, and slope. Compared with "law," a pattern is relatively less fixed and determined: a pattern is a regularity rather than a rule. A pat-

tern consists of more than one element arranged in some systematic manner, such that one element in the pattern presupposes the other. This arrangement, or grouping of elements in a more or less regular fashion which tends to persist as a unit, is the distinctive feature of a pattern. The emphasis of those concerned with patterns is in the structuring of the elements, the recurrence of the pattern as a unit, and the relative strength of the interlocking elements which compose the pattern.

In the context of a single culture, a pattern has been conceived of as a model or guide—"something which someone follows in making something" (Radcliffe-Brown 1952:14). This notion of pattern is exemplified best in dress where, as Kroeber (1948:331) points out, "the first association of many women to 'pattern' is likely to be that of a paper model from which dresses are cut and shaped." That kind of order which applies to the pattern concept in general applies with equal force to the pattern thought of as a model.

In the present study, the pattern concept is limited in meaning to a model or a guide. Further, a pattern is portrayed as a dynamic concept; in this sense it becomes a procedure; that is, interlocking steps in a plan for getting something done. Thus, the following ethnographic description of the manufacture of a pounder on the island of Ua Huka in the Marquesan Islands is taken as a model for making a pounder.

(1) An irregular piece of stone about three times the size of the finished implement is roughed out with heavy blows of the ax until it becomes a cone. . . . (2) This cone is then worked down by light blows on all sides until the general form of the finished implement is produced. (3) This is then smoothed by light, even blows with the corner of the ax until all the contours are perfect. (4) The neck and body of the implement are then shaved with the edge of the ax until the scratches left by the last process are obliterated. The head and base are left rough. . . . (5) The body of the implement is sometimes rubbed with a mixture of coconut charcoal and oil which gives it a shiny black finish contrasting pleasantly with the grey of the head (Linton 1923:337-38; insertion of numbers mine).

The ethnographic description of the manufacture of the pounder is a generalization grounded on the observation of on-going actions in the present. A man in a given day, for example, may make two items, a pounder and an "X." His actions with regard to pounder-making are separated from his actions with regard to making "X," coupled with the actions of others with regard to pounder-making, and generalized with the result that a model of pounder-making is detailed. The generalization is subject to test by further observation. Since some men sometimes polish pounders and some men sometimes do not (see step 5 above), the model is a description of a regularity.

In order to formulate a dynamic pattern, analogous with the ethnographic one, the archeologist would have to construct a sequence of manufacture. This construction would necessarily be based on the observation of actions as materialized or congealed into static forms. Thus, parallel with the above example, the archeologist might observe all the recoverable objects on Ua Huka, classify the objects into separate groups on the basis of features which are believed to be diagnostic, select out a group of bell-shaped objects (pounders) which he believed were manufactured in the same way, select out another



group of objects (axes) which he believed could have been used in the manufacture of the bell-shaped objects, and, finally, construct and test a pattern of manufacture.

Although the forms in the archeological parallel are only two, bell-shaped objects and cutting tools, and the problem posed is manufacture, patterns may include other kinds of forms and problems. Consider, by way of example, two of the imitative experiments presented in the previous sections. The position of the jar relative to the outcropping, in the example in the second section, is a spatial arrangement relevant to the pattern of use, where the forms include a jar and a natural formation (outcropping). The case involving the scapulae and ribs (Case 3) is a problem which simultaneously involves use and manufacture. The sequence in this pattern, as constructed by Morris and Burgh, includes: 1) stripping the edges off a yucca leaf; 2) drawing the leaf across a peeled pole while hammering with a cobblestone; 3) scraping the leaf with a rib to remove the fibre from the pulp; 4) immersing the fibre; and 5) repeated scraping of the fibre with a rib. The sequence produced a notched rib, but a notched or an unnotched rib may be used in producing the fibre. Hence, the item, which might be a product of use in one instance, may become usable as a tool in another instance. The forms in this pattern include a yucca leaf, a peeled pole, a cobblestone, a fluid (kind not given), and a rib.

Although the particular forms and problems which constitute the subject matter of imitative experiments are varied, all have as a prerequisite the classification of artifacts into groups. In Case 2, for example, grooved, oblong stones had been divided into two groups on the basis of hardness of stone. The division was believed to be indicative of use: soft, grooved, oblong stones serving to smooth arrow shafts; hard, grooved, oblong stones serving to straighten them. In the experiment, Cosner might have tested the hypothesis that each class was used for a different purpose. However, he centered his attention on the belief that the class of harder objects had been used to straighten arrow shafts. The pattern he constructed in the experiment to test this belief included forms other than artifacts; for example, he used arrow weed and reed cane. The particular hard, grooved, oblong artifacts which he did use were real objects but, presumably, they were truly representative of the class of objects under examination. The same notions apply to other experiments: the designations "charmstone," "notched rib," "usable lump of copper," and "paintings" are tag names for classes or types of objects or representations, and the particular artifacts used in experiments are taken to be typical of those classes.

Approaches to formulating or grouping artifacts into classes have been a major theme in archeological literature for the past 30 years. Discussion has pivoted about the dual interest of archeologists in reconstructing cultural contexts and in establishing spatial-temporal relations. When interest is centered primarily on reconstructing cultural contexts, the objects recovered by the archeologist tend to be treated as "expressions of ideas and behavior of the people who made them" (Phillips, Ford, and Griffin 1951:61). When interest is concentrated primarily on spatial-temporal relations, the objects become

instruments or markers used in the establishment of these relations. As Phillips, Ford, and Griffin have suggested: "one's approach to the problem of classification will depend largely on which of them [cultural contexts or spatial-temporal relations] is being served."

Those who have approached classification primarily as a means to aid in the reconstruction of cultural contexts, regardless of technique (Krieger 1944: 278; Rouse 1939:18; Spaulding 1954:392), apparently accept the following general proposition, in one form or another: Artifacts produced from the same scheme, or used according to the same scheme, exhibit similarities which permit their division into groups which reflect those schemes. Imitative experimenters work with classes developed on the basis of this proposition even if they do not always develop those classes themselves. This proposition, therefore, constitutes an implicit theoretical and methodological base for the execution of imitative experiments. The reasoning behind this proposition may be demonstrated by reference to the ethnographic example of pounder-manufacture given above.

Any individual on Ua Haku Island, familiar with the pattern for making a pounder, presumably would produce one which would look like, or be similar to, a pounder produced by another individual. Incomplete transmission of the pattern from generation to generation or within a generation, variation in manual dexterity, availability of proper materials, and other factors would result in observable variation in completed pounders. Nevertheless, the pounders produced according to the pattern for pounder-making would, presumably, look more similar to each other than to other objects produced in accordance with other patterns. Unquestionably, various factors, as indicated, would result in a range of physical properties of pounders: certainly no two would be identical.

If the above is true, then the archeologist should be able to separate bell-shaped objects from other objects and construct a class "bell-shaped object." Further, he should be able to choose one or a few type-specimens, where a type-specimen is defined as "*a real object designated to represent a class and serving as the basis for scientific description*" (Osgood 1942:23), or in the present context, serving as a basis for experiment. It need hardly be pointed out that any classification is an hypothesis. Classification is an "experimental activity of trial and error . . . there is no test for what is like and what is unlike except an empirical one" (Bronowski 1959:58). Taylor (1948:123), in particular, is insistent on this point: "There is no automatic, axiomatic assurance that the forms, types, and classes established today by the archaeologist are coextensive with any separable entities that existed in the minds or life ways of bygone peoples . . . any such correspondence is a matter for explicit hypothesis and testing, not implicit assumption."

Each imitative experiment is an attempt to test a belief about cultural behavior, relying implicitly on the first proposition: all cultural behavior is patterned. The statement of the hypothesis describing a particular pattern involves artifact classes and has implicit within it the second proposition: arti-

facts produced from the same scheme, or used according to the scheme, exhibit similarities which permit their division into groups which reflect those schemes. Taken together, the two propositions form the implicit broad working hypothesis of the imitative experiment.

#### THE LOGIC

At first glance the imitative experiment appears so unlike experiments in other disciplines that it becomes suspect. For example, the kind of order (pattern) with which imitative experimenters are concerned is cultural, not natural; hence it is not like an experiment in the natural sciences. The fact that the patterns existed in the remote past suggests that the imitative experiment is unlike experiments in the social sciences. If experimentation were defined by the order which is involved or the temporal locus of that order, it would be concluded that the imitative experiment is not really an experiment but is something else.

Experimentation is really a kind of activity which is common in daily experience as Conant (1950:7) has recently pointed out. An experiment is performed when a person tries to unlock a door with a key which he has not previously tried. In doing the experiment, the person may be thought of as saying to himself "If I turn the key, the lock will spring." In this case, according to Conant, "common-sense assumptions and practical experience determine the nature of the experiment. . . ." In more sophisticated experimentation "a series of connecting links usually relates some deductions of a broad working hypothesis with the final limited working hypothesis involved in the specific experiment."

In the present context, the broad working hypotheses are the general propositions detailed under theory. The limited working hypotheses are the ideographic statements made possible by the general propositions. The experiment is the testing of these ideographic statements. It is important to note that the general propositions are not under scrutiny in the experiment: the tenability of these propositions can be examined only in a contemporary ethnographic situation.

The essential ingredients in testing a limited working hypothesis, according to Peirce, consist of the following:

First, of course, an experimenter of flesh and blood. Secondly a verifiable hypothesis. . . . The third indispensable ingredient is a sincere doubt in the experimenter's mind as to the truth of the hypothesis. Passing over several ingredients on which we need not dwell, the purpose, the plan, and the resolve, we come to the act of choice by which the experimenter singles out certain identifiable objects to be operated upon. The next is the external (or quasi-external) ACT by which he modifies those objects. Next comes the subsequent *reaction* of the world upon the experimenter in a perception; and finally his recognition of the teaching of the experiment (Peirce 1934:424).

Peirce goes on to say that "the unity and essence of the experiment lies in its purpose and plan, the ingredients passed over in enumeration," but he does not elaborate on either. The purpose of the imitative experiment has already been detailed, the plan has not. An experimental plan is here conceived as the principles of reasoning or logic which facilitate the testing of a limited working

hypothesis. The ingredients mentioned by Peirce are interwoven within that logic.

Consider the formal structure in the example given by Conant. The statement, "if I turn the key, the lock will spring," can be generalized to read: "If I do A, I will get B." Compare this with ethnographic pounder-making on Ua Huku presented above. In the first stage in the pattern, an irregular piece of stone is roughed into a cone with blows from an ax. The pounder-maker knows that if he applies the ax to a piece of stone in a certain manner he will produce a cone. Learning the pattern is, in effect, learning how to get certain results by performing certain operations. In the first stage he may be thought of as knowing "if I apply the ax to a piece of stone in a certain manner, I will produce a cone." The formal structure in this statement can be generalized in the identical manner as the statement in Conant's example: "If I do A, I will get B."

Conant's example and the first stage in pounder-making appear to differ in that the former is complete and the latter is a first stage in a five-stage pattern. The operations which the man at the door performs, however, can be viewed as a sequence. The placing of the key in the lock, for example, is a step which precedes the turning of the key. The operations of the man at the door can be viewed as a set A', where A' represents all the steps in the sequence relevant to the problem of opening the door. The five-stage pattern in pounder-making similarly forms a set A', where A' represents all of the related stages relevant to the producing of a pounder. Thus, the structure of both statements retains formal identity; "If I do A', I will get B."

The example of opening the door and pounder-making is evidently different in other respects. The man at the door is testing the key in order to see if it works: the resident of Ua Huku knows that following the pattern will result in a pounder. Testing if the key will open the locked door is an experiment; making a pounder is not. It is the parallel formal structure of both statements, however, which supplies rationale for imitative experiments.

The manner in which the archeologist would handle a collection which included bell-shaped objects (pounders) from Ua Huku has already been detailed. Let it be assumed that the archeologist has grouped bell-shaped objects into a single category on the basis of what appears to be a common scheme of manufacture. His limited working hypothesis might be: roughing, shaping, smoothing, shaving, and polishing (or not polishing) produced the bell-shaped objects. The five steps in the proposed sequence can be grouped into a set A'. If the archeologist chose to test his hypothesis by experiment, he would have to convert his limited working hypothesis into a verifiable hypothesis of the form "If I do A', I will get B." This is clearly verifiable for, in performing the operations, the archeologist either will or will not manufacture a pounder. Just as clearly, it is an experiment: the archeologist does not know if his converted hypothesis will pass the tests.

Examination of the cases of imitative experiments shows that initial

limited working hypotheses are not always as complete as "roughing, shaping, smoothing, shaving, and polishing (or not polishing) produced the bell-shaped objects." An experiment will often begin with a statement of the form "notched ribs and scapulae were used to extract fibre from pulp," and the development of a sequence of use and/or manufacture will progress as the experiment progresses.

The progressive development of an experiment is limited and directed by a number of guides. If it were not for these guides, scant confidence could be placed in an inference based on an experiment, for, it could be argued, the experimenter could achieve B by one means or another. There are three guides which both limit and direct an experiment.

The first guide is evident in the choice of objective material. The objective material is here defined as the subject for study in the experiment. The objective material must be known to have been available, or could have been available, in the aboriginal setting. In Case 3, for example, the objective material is a notched rib from the site and fresh ribs. In Case 1, the objective material, wax resin, marrow fat, mutton fat, hyrax urine, plant juices, bile, honey, and tempera, were available in the site area. (For further examples of the choice of objective material with this criterion, see Nero 1957:303; Osgood 1942:33; Outwater 1957:261; Rau 1869:394; Smith 1953; Smith and Watson 1951:18.)

The second guide involves the choice of the effective material. The effective material is that material which is used to produce a change in the objective material or is changed through the use of the objective material. The effective material must be, or simulate, a means available to aboriginal people, or be in accord with a material which is known to have been or could have been available in an aboriginal setting. In Case 2, the means of producing a change in the steatite and sandstone (objective material) are tools (effective material) from closely affiliated archeological sites. In Case 4, the objective material (arrow-shaft straightener) produces a change in the effective materials (reed cane and wood). (For further examples of the choice of effective materials with this criterion, see Farnsworth and Wisely 1958:165; Griffin and Angell 1935:1; Iversen 1956:37; Kent 1957:464; McEwen 1946:111; Voce 1951:113.)

The third guide is the physical characteristics of the objective and effective materials. Unlike the first two guides, the third is not self-imposed; it is prescribed in the materials. An experimenter must, for example, work within the bounds of the hardness of steatite (Case 2) and the fibre-pulp binding in yucca leaves (Case 3).

The experimenter, forced to work within the limits of what is given in nature and what was, or could have been, available in an aboriginal setting, is also directed in his operations by those limits. In Case 5, for example, the smelting temperature (physical characteristic) of copper oxide ores (objective material) directs the experimenter to some means available to aboriginal peoples to effect smelting. The hole in the ground furnace (first effective material) cannot produce that temperature and is rejected. The model kiln (sec-

ond effective material) can produce the temperature and is simulative of kilns known to have been used. (For further similar examples, see Cosner 1956; Ellis 1940; Fowler 1946; Hawley 1953; Knowles 1944.)

At any step in the progressive development of an experiment, the experimenter might attempt to work with one or more experimental materials or attempt a number of operations with the same or different materials and find that either: (1) only one operation is possible; or (2) more than one operation is possible. A set is composed of a sequence of steps; if one or more of the steps is different it defines a different set. Thus, if the experimenter could achieve his end (B) via only a single sequence he could observe: of the alternatives  $A'$ ,  $A'' \dots$ ,  $A^{(n)}$ , if I do  $A'$  and only  $A'$ , I will get B. If, on the other hand, he found that he could achieve B via more than one sequence he could observe: of the alternatives  $A'$ ,  $A''$ ,  $\dots$ ,  $A^{(n)}$ , if I do  $A'$  or  $A''$ , I will get B.

Converting the limited working hypothesis from a statement of what was believed to have happened in the past into a form that can be tested in the present establishes a verifiable hypothesis. The choice of experimental materials to operate upon and with, and the subsequent operations lead to an observation. The interpretation of this observation may be as follows:

If the experimenter observes that he can achieve B by one and only one set, he can convert his observation into an inference. The success of the experimenter in achieving B does not imply that B was achieved in the past necessarily in the same way as did the experimenter. The experimenter's success does mean that B could have been achieved in the manner indicated by the limited working hypothesis.

If the experimenter observes that he can achieve B by more than one set, he can still formulate an inference. The fact that the experimenter could achieve B in more than one way does not negate this interpretation because there is nothing necessary about the kind of order (pattern) with which the experimenter is concerned. Further, he is not attempting to discover how a people did achieve an effect, he is testing whether or not they could have achieved an effect in the manner indicated by the limited working hypothesis. The development of two equipossible sets, each of which could result in B, is parallel to searching the literature for ethnographic analogies and finding that two or more analogies can serve to transform an hypothesis into an inference.

The experimenter, finally, may find that by none of the alternative sets tried can he achieve B, and, hence, cannot formulate an inference. In this case he would re-examine his original classification, restate his limited working hypothesis, and begin the experiment once again. If he failed in successive attempts, he could formulate no inference on the basis of test by experiment.

The process of performing an imitative experiment may be summarized as follows:

- 1) Converting the limited working hypothesis into a verifiable form.
- 2) Selecting the experimental materials.

- 3) Operating with the objective and effective materials.
- 4) Observing the results of the experiment.
- 5) Interpreting the results of an experiment in an inference.

Confidence in an inference based upon an experiment can be increased in three ways: (1) by choosing experimental materials which were, or could have been, available in an aboriginal setting; (2) by finding corroborative evidence; and (3) by performing as many alternative sets as is feasible. Restricting experimental materials by the criterion of the aboriginal setting increases confidence by limiting the operations which the experimenter can perform to those which were possible for the aborigines. The finding of corroborative evidence suggests that the operations which the experimenter performed might have been performed in the past. Thus, the recovery of a notched rib imbedded with pulp and sap (Case 3), presumably after the experiment was completed, suggests that the experimenter and the aborigine used notched ribs in a similar way. By executing a number of alternative sets, the experimenter may eliminate some and find others which are equally possible. This process increases confidence in the same manner as a library search for suitable ethnographic analogies would: some analogies, believed to be relevant at first, are found wanting; others, previously or not previously considered, are found to be applicable. In the context of the imitative experiment, the number of alternative sets which the experimenter performed is probably the only measure of what Peirce calls "sincere doubt in the experimenter's mind of the truth of the hypothesis."

The recognition of possible relationships between objective and effective material and the awareness of the physical characteristics of those materials depends on the knowledge and capabilities of the experimenter. The execution of the operations depends upon the experimenter's skill. Knowledge, capabilities, and skill are subjective in the sense that all individuals do not possess them in equal quantities or proportions. It cannot be denied that these elements play a role in the performance of imitative experiments. On the other hand, these elements are present in any endeavor that involves the recognition of order among facts and the testing of that order. Clearly, the same subjective elements are involved in experimentation in any of the social and natural sciences (Cohen and Nagel 1934:245-72; Goode and Hatt 1952:76-80).

Criticisms of the imitative experiment have focused on the contention that the results of a particular experiment are not conclusive. When such criticisms are made, the critics generally suggest that the item or items in question could have been manufactured or used in a different way. (See, for example, Moorehead's [1936] criticism of experiments with bone points performed by Tyzzer [1936] and Tyzzer's [1937] reply.) In formulating their arguments, authors may introduce the differential nature of archeological preservation, differential geographical distributions, observations made in a protohistoric or historic setting, and parallel but different experiments. (See, for example, Over's [1937] and Ray's [1937] critiques of Cox's [1936] experiments

with scrapers.) In certain cases the arguments are particularly persuasive. (See Fenenga [1953] on experiments with projectile points executed by Browne [1938, 1940] and a later experiment performed by Evans [1957].)

No belief can be established with finality and no knowledge is based upon knowing all the facts. Neither analogy, the imitative experiment, or any other tool which the archeologist now has at his disposal, can be used conclusively to establish a belief about the past. The archeologist must work with what Pareto (1935:319) has called "facts in scant numbers." The challenge of archeology is in transforming hypotheses based upon scant data into legitimate inferences. It is hoped that the results of this study will encourage archeologists to use one of the few mechanisms which can secure such transformations with confidence and clarity.

## NOTE

<sup>1</sup> The initial stimulation for doing this kind of study came from reading Raymond H. Thompson's (1958) work on inference in archeology. I am particularly indebted to Clement Meighan, my teacher, for his encouragement, suggestions, and comments. Others who in various important ways have contributed to its present form are Marcia Ascher, Ralph Beals, Joseph Birdsell, and Makato Kowta.

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## Brief Communications

### SORCERY ON IFALUK

A few years ago, in reviewing *An Atoll Culture: Ethnography of Ifaluk in the Central Carolines* by my esteemed friends, Edwin G. Burrows and Melford E. Spiro, I noted that the authors had reported the absence of sorcery on Ifaluk. For two reasons, it was hard to accept their findings. First, our general knowledge of supernatural systems of belief tells us that sorcery is almost universal throughout the primitive world; secondly, black magic has been reported for other islands in this part of Micronesia. After fretting for some time over this inconsistency, I feel that I am now in a position to offer data indicative of the presence of aggressive magic in the Ifaluk scheme of things. The implications of my findings go beyond the mere correction of a field report, and Professor Spiro may want to revise his theory of the role of evil spirits as an outlet for personal hostility in the atoll in question (see Spiro 1952). It is true that my evidence does not come from first hand research on Ifaluk itself, for I have not been able to visit that island group during my current field trip to Ulithi Atoll, where this communication is being written. But the two atolls are close both geographically and culturally, and the reader can judge for himself the value of findings uncovered through the indirect means I have employed.

My presentation is chronological. In discussing sorcery with two highly reliable informants, I bluntly raised the question of its reported absence on Ifaluk. The younger man, Iamalamai, immediately expressed astonishment at the allegation. Pressed for documentation of his skepticism he could offer little more than an opinion derived from his general knowledge of this part of the Carolines, as well as visits to Ifaluk itself; but even though I subjected him to close questioning in my search for something more substantial and he was unable to supply it, he remained unshaken in his conviction that his position was tenable even if derived from little more than his knowledge of the over-all situation as he knew it from a few specific places. Then, unexpectedly, for I had never had an open admission of sorcery on Ulithi even after two previous field trips, he cited an instance to show that the black art could be prevalent and yet concealed from the outsider. The example he chose was discrete because it involved two sorcerers from Eauripik and Woleai, respectively, but their conflict had occurred on Ulithi after each had become jealous over the other's skill in canoe building. I listened to this admission with some amusement, for I had previously been led to form the conclusion that the strong palliatives against black magic on Ulithi were to protect one against Yapese sorcerers; but I had read between the lines, as it were, and had accepted this as a diplomatic Ulithian way of avoiding a head-on meeting of the issue.

The older informant, Melchethal, wasted no time on opinion but went straight to the facts. He had made three trips to Ifaluk, the first being about

the mid-thirties during the Japanese administration when he went with his wife to see her maternal relatives. He stayed approximately a year. The second and third times were in the earlier years of the American administration, and on each of these occasions he stayed about three to four months. It is noteworthy that his wife's mother is from Ifaluk, although she had died before he ever went there. It should be apparent that these three trips have given Melchetal, a highly observant and well-informed man, a reasonably intimate familiarity with Ifaluk, which in any case has always had a fair degree of interchange with other islands.

On the occasion of his second trip, which I estimate to have been about 1954, he heard that during the war with Japan, two men from Ifaluk named Ienefel and Therulubwong were on Yap and for some reason became mutually embittered. They performed sorcery against each other. Therulubwong died immediately on Yap, while Ienefel died some months later on Ifaluk. Both had abdominal ailments. On being asked by me if the men might not have learned their sorcery on Yap, my informant said that this was impossible because such magical ritual is passed on only from parent to child. On being asked if the men might not have hired Yapese to do their work for them, he said that while such vicarious aggression is possible this is not how he heard the account on Ifaluk. Without hesitation, he then told of another case he had heard on the occasion of the same visit. A man named Rolemar who lived on the atoll tried repeatedly to seduce a married woman. She would continually agree to meet him in the woods at night but would never keep the rendezvous. After each occasion Rolemar would see her and complain, but she would retort that she had gone to the assigned spot and left when he did not appear. Once, after arranging another tryst, the woman told her husband about the matter. The husband went to the place and put on a wrap around skirt to give the impression he was his wife. When Rolemar approached and discovered that he had been tricked he became so angry that in retaliation he performed black magic against the woman. She died, apparently after a few months, but the sorcerer himself had died shortly before, fulfilling a Carolinian belief that those who sorcerize others are themselves doomed to die—a matter whose logic we need not explore here. Rolemar had a daughter, and she lives permanently now in the village of Mogmog where I make my headquarters while on Ulithi. When my informant suggested that that we should interview her, I decided against doing so on the grounds that it would be indelicate to discuss such an embarrassing episode with a person so closely involved.

Melchetal added that he had become a Christian many years ago and had become so imbued with his new religion that he had accepted an invitation by the new missionary for the islands of the area to convert the people of Ifaluk. This he tried to do during his second visit. Among his arguments to them he said that the people should give up sorcerizing because it was this that made Ifaluk more ridden with yaws and other diseases than other islands. They admitted the great prevalence of black magic and promised to mend their ways. We must note, then, that the matter was openly discussed. Melchetal says

that in doing so he repeatedly used the Ulithian word for sorcery, *hasupsup*, as well as its Ifaluk variant, *hososou*. (On asking him how he reconciled his present disbelief in sorcery with the use of the concept to admonish the people of Ifaluk, he said that he recognized the confusion in logic but had resorted to expediency in order to correct a bad situation.) In any case, there were no protestations of innocence.

Two days after the above interview there came additional verification of the presence of sorcery on the atoll. Melchethal reported that over the week end he had decided that it would not be inappropriate after all to question Rolemar's daughter, whose name is Ilchemal, and had accordingly interviewed her at some length. I have trained Melchethal, who has been my principal informant during two field trips to Ulithi, to do questioning for me, and I have complete confidence in his ability and integrity; but in this case I grilled him step by step as to his method of extracting the information, his accuracy in remembering the facts, the veracity and reliability of the informant herself, the names of the persons involved, and so on.

In somewhat condensed form, this is what he reported back to me: The woman, named Ilchemal (whose age I estimate to be about thirty-five), said that both white and black magic exist on Ifaluk and are the same as on Ulithi. On being pressed for instances, she at first pleaded ignorance, but after some persuasion from Melchethal, who is an elderly man and a relative by marriage, she told of a family that several times moved from one place to another on Ifaluk after one of its members became ill or died. This was because it suspected sorcery but could not pinpoint the magician and therefore took this diffuse method of protecting itself. The family had even gone to the trouble of performing divination through the complicated system of palm leaf knots, known in the Carolines as *bwe*. After some further pressure had been put on her, the woman recalled another instance. A female chief called Lawichmel (who is related to Melchethal's wife) went one day to the swamp garden and stepped into another woman's taro patch. The other woman, whose name was Ilemelewer, took the soil the chieftess had stepped on and performed a black ritual over it. The leg of the chieftess became greatly swollen from elephantiasis. The informant said that this story, and the one to follow, were told to her when she was a young woman, at the close of the Japanese administration. There was a beautiful woman named Letamelsei. Her relatives used to sing songs extolling her great beauty, and would even follow her about, singing. This created criticism among the people of Ifaluk, so these relatives called a halt to their singing. But shortly afterwards the girl became afflicted with yaws and lost the flesh from her nose. The people said that this was the work of a sorcerer, although it was impossible to suspect any one person because so many people had been jealous of the girl. Then the informant came around to mention the death of her own father, Rolemar. When he became fatally ill, Ilchemal was a pre-pubescent girl and when she would attend to her father she would hear those gathered about say that he had been sorcerized. He had a swollen leg that exuded much pus (it was not elephantiasis), and he died after two or three

months. Beyond this, she added nothing. The question of Rolemar's own sorcery against the woman who had tricked him was not brought up. The interview between my worker and this informant ended with her assertion that since the present missionary began his activities on Ifaluk, there was no more sorcery, but that before this she had been one of the only eight Christians on the atoll, the others being a man named Uichilior and another named Tawerier, together with his family. It should be kept in mind that the conversion of the people of Ifaluk began after Burrows and Spiro left. Melchethal says that when he went there around 1954 to aid Father Walter in his work, he prepared thirty-seven or thirty-eight people for acceptance into the Church.

There is little doubt that the kinds of evidence I have presented could be expanded by further research on Ulithi, but I shall make only one more observation. After the above data had been collected, I chanced to note that in a tale being told to me by a storyteller, the people of Ulithi engaged a man from Ifaluk to perform some black magic for them. On asking the narrator, who knew nothing of my interest in the matter in question, why they had appealed to an outsider, she answered that it was well known that the natives of Ifaluk and Satawan-itu are the most skilled magicians of all, and that people from other islands know they perform sorcery.

If, then, black magic did exist on Ifaluk in 1947-1948 when Spiro did his research, it would undermine his argument that the people of the atoll, who must avoid open social aggression because intimate cooperation is indispensable in a tiny society, have shunned sorcery and turned to the *alus*, or spirits, in giving release to their hostilities. While I am willing to agree that spirits may have psychological value as scapegoats, I feel that sorcery is fully as important—at least for the time in question, for matters on both Ulithi and Ifaluk are undergoing rapid changes due to acculturative influences. Psychological hypotheses concerning psychological behavior often rest on speculative interpretation of facts, but in this case we are dealing with a question of the veracity of the facts themselves. It would be interesting to know what Professor Spiro, a colleague I hold in high respect, has to say on the subject.

Ulithi Atoll

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#### SORCERY, EVIL SPIRITS, AND FUNCTIONAL ANALYSIS: A REJOINER

I am not at all sure that my comments will be as "interesting" as Professor Lessa hopes, but as a partner in the Lessa-Spiro mutual admiration society and



as a proponent of "psychological hypotheses" (as he calls them) I feel that I should at least attempt to deal with the issues he raises in his letter.

As Professor Lessa properly indicates, his letter raises two issues: factual and theoretical. As an incorrigible theorist I must hasten to confess that my interest in facts, even important facts, is somewhat less than enthusiastic; whereas almost any theory, even a bad one, can easily attract—if not always hold—my attention. In this instance, however, I can hardly feign indifference to the facts which Professor Lessa calls to our attention because, if they are correct, I would have to admit to sloppy field work—an admission to which even a self-styled theorist is unhappy to confess. At the risk of such an admission I can only reiterate what Professor Lessa already knows, and which is the point to which he addresses his comments, viz., that Burrows and I were unable to discover any evidence for sorcery in Ifaluk, and that this negative finding was reported in our monograph. Lessa's recent findings cast serious doubt on the accuracy of our report. I say "serious doubt," not from a desire to hedge by the use of vague rhetoric, but because his findings, although substantial, are still circumstantial. It is not at all difficult to believe, for example, that persons who live in a society (in this case, Ulithi) in which sorcery is present should attribute this practice to members of other societies as well; or that emigrés should reinterpret events which occurred in their natal environment in terms of the cultural beliefs of their new environment. Nevertheless, circumstantial or not, Professor Lessa's findings certainly seem convincing, and I can assure him that I shall take another long and hard look at Ifaluk sorcery during my impending return trip to that atoll.

The really important issue, however, for both Lessa and me—and which, I would suppose, is the only justification for the publication of this exchange—is the theoretical one. For what is at stake in this controversy concerning the existence or nonexistence of a cultural practice in an isolated and obscure tiny Pacific atoll is the strategy of functional analysis and the validity of "psychological hypotheses" which, Professor Lessa implies, are in some sense uniquely "speculative"—in contrast presumably to nonpsychological hypotheses which are less speculative and stand on a firmer empirical base. In this instance, however, the actual problem concerns the logical structure of a theory, rather than the empirical foundations of the theory, and on this score our differences are fundamental. If sorcery does in fact exist on Ifaluk does it follow, as Professor Lessa suggests, that I "may want to revise" my functionalist explanation of Ifaluk evil spirits? Would this fact, to employ his expression, "undermine" my argument? Would the "psychological hypothesis" be disconfirmed? My answer to all these questions, in direct and explicit contrast to his, is unambiguously no. In order to understand our differences, we must first examine the logical structure of my argument.

In my article I stated that the Ifaluk belief in evil spirits (and the rituals associated with this belief) is a cultural mechanism which—whatever the causes which may have given rise to this belief—is used by the Ifaluk for the displacement of hostility; this is one of its *latent functions*. I further argued

that, although the deflection of some hostility from the in-group is a functional requirement for any society, it is particularly critical in a society which, like Ifaluk, inhabits a minuscule, physically bounded, land mass, whose economic system is predominantly "cooperative," and whose value system strongly disapproves of interpersonal aggression. Nowhere, however, is it either stated, suggested, or implied that this functional requirement can be satisfied by only one structural unit—in this case, the belief in evil spirits. In short, nowhere is it affirmed that the belief in evil spirits is a *necessary* condition for the displacement of hostility. (Lessa's failure to distinguish between theories which stipulate either necessary conditions or necessary and sufficient conditions, on the one hand—which mine does not—and those which stipulate only sufficient conditions—which mine does—seems to be the basis for his critique of my argument.) Quite the contrary, on page 500 and again on page 502, the article stresses the fact that numerous structural units, including sorcery, may serve this same function; that, in short, there are functional equivalents for the belief in evil spirits. In sum, although the article explicitly argues that the displacement of hostility from the in-group is a *functional* necessity for any society, it explicitly denies that the belief in evil spirits, as a means for serving this function, is a *structural* necessity, in Ifaluk or in any other society. (Indeed not only does the article not postulate the belief in evil spirits as a necessary mechanism for the displacement of hostility, it does not even suggest that it is a *sufficient* mechanism. It is obvious, however, from the exclusive attention devoted to this belief in the article that, among the various mechanisms in Ifaluk which serve this function, I considered it to be the most important.)

Finally, and because this article deals with primitive religion, I compared the functional consequences of in-group sorcery, which had been suggested by more than a few distinguished scholars as a means for displacing hostility, with those of the belief in evil spirits.<sup>1</sup> (The contrast is with in-group rather than out-group sorcery because, with respect to the one function—displacement of hostility—with which we are concerned in this discussion, sorcery against the out-group, especially if the out-group is perceived as evil, is functionally identical with belief in, and the practice of rituals attacking, evil spirits.) If sorcery and evil spirits alike provide a socially approved outlet for hostility, i.e., if both structural units serve this same functional requirement, are we to say that these structural alternatives are functionally equivalent? Or can it be argued that one of these structural units is a more adequate means than its structural alternative for the attainment of this functional end? I argued then, and I would argue now, that on this criterion the belief in evil spirits is functionally superior to sorcery. For although the belief in sorcery, like the belief in evil spirits, allows displacement of hostility from persons whom one is expected to love or respect to sorcerers whom one can hate with impunity (because they are evil), it differs from the latter belief—and this is especially so if it is believed that anyone is a potential practitioner of sorcery—in that it also evokes hostility toward and insecurity concerning one's fellows. If this is so then the net-

balance of functional consequences, as Merton would put it, would certainly tip the scale in favor of evil spirits.

Having stated the argument, it may now be asked if the presence of sorcery in Ifaluk would affect either the evil spirit hypothesis or my assessment of the relative functional merits of evil spirits and sorcery. As for the hypothesis it may, of course, be false on other grounds. But assuming—and Professor Lessa too is willing to accept this assumption—that the hypothesis is not false on other grounds, I fail to see how either the absence or the presence of sorcery in Ifaluk affects its validity. Since the belief in evil spirits is not postulated as a necessary condition for the displacement of hostility, and since, *ex hypothesi*, numerous structural alternatives, including sorcery, may individually or severally have this functional consequence, the presence of sorcery would merely mean that in Ifaluk (as in most societies) two or more cultural mechanisms serve the same functional end. If anything, the existence of sorcery in Ifaluk would strengthen rather than weaken the validity of the evil spirit hypothesis. For if sorcery has the effect, as I have argued, of provoking hostility and if—as in the case of Ifaluk—interpersonal hostility is prohibited, sorcery would enhance rather than diminish the functional importance of evil spirits as a means for its displacement.

But I would not press this point. To return rather to my first point, since I argued that the belief in evil spirits is only one of the mechanisms by which hostility may be displaced, and since I explicitly designated sorcery as another, I can hardly agree with Professor Lessa that the existence of sorcery in Ifaluk—even if it were “fully as important” as evil spirits—should require that I “revise” the evil spirit hypothesis.

This leads us to Professor Lessa’s final argument that if sorcery had existed on Ifaluk at the time of our study it would “undermine” the thesis that the Ifaluk “have shunned sorcery and turned to the *alus*. . . .” To this I can only say that, even if it would undermine *this* thesis, it would not undermine *my* thesis because, although Professor Lessa attributes this thesis to me, it is not mine. Nowhere in the article is it either stated or implied that (a) the Ifaluk “turned to” spirits to satisfy certain functional requirements of their society, or that (b) by “turning” to spirits they would have to “shun” sorcery. Neither proposition is either part of the theory or deducible from it.

First, since the displacement of hostility is interpreted as a *latent* function of the belief in evil spirits, I could not possibly argue that this belief either arose or persists “because intimate cooperation is indispensable. . . .” The argument asserts, instead, that however it arose and by whatever conditions it persists, this belief does in fact have this functional consequence (causes, motives, and functions are empirically and analytically distinct variables). Second, since the concept of functionally-similar and functionally-equivalent structures is built into the very core of the theory, I could not possibly hold that the presence of spirits entails the absence of sorcery. On both accounts my argument is quite different. Given the Ifaluk economic-ecological-value configura-

tion described above, successful hostility displacement, I reasoned, assumes even greater importance in this atoll than in other societies. And since sorcery, as I attempted to show, has certain dysfunctional consequences which it does not share with evil spirits, the use of evil spirits rather than sorcery as a displacement mechanism was, I argued, consistent with—not that it followed from—the displacement hypothesis. This argument most certainly does not imply—as Professor Lessa seems to think when he suggests that the presence of sorcery would “undermine” the hypothesis—that the hypothesis would predict the absence of sorcery in Ifaluk. It would not. It would predict rather that, given the Ifaluk configuration, the evil spirit belief could exist, either alone or together with sorcery, but that sorcery—because of its dysfunctional consequences already alluded to—could not exist alone.

Nevertheless, although the existence of sorcery does not affect the displacement hypothesis, it does affect my general assessment of the Ifaluk social system and Ifaluk character. If nothing else—and for this I am grateful to Professor Lessa—the existence of this disruptive force will compel me to look for other integrative mechanisms in that society in addition to those I had already singled out (in other publications).

May I conclude by remarking, since my respect for Professor Lessa is certainly as high as his for me, that I do not regard the logical pitfalls inherent in his critique of “psychological hypotheses” to characterize his position uniquely. These same logical fallacies can be shown, I believe, to inhere in many of the criticisms leveled against culture-and-personality and functionalist interpretations of social systems. I am grateful to him, therefore, not only for calling my attention to a possibly serious error in the Burrows and Spiro monograph, but also for the opportunity of explicating part of the logical structure of culture-and-personality functionalist theory.

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#### ON ANCESTOR PROPITIATION AMONG THE MAPUCHE OF CENTRAL CHILE

Central to Mapuche (Araucanian) religion and the moral order of society is the concept of a continuing relationship of mutual dependence between the living and their ancestors. Features of this relationship have been recognized by Chilean historians, folklorists, and ethnologists during 400 years of contact with the Mapuche. The systematic propitiation of ancestral spirits has recently been ignored by Sister Inez Hilger and denied by Mischa Titiev (Hilger 1957; Titiev 1951). Since the “raw data” of both these authors correspond closely with my own, I would like to comment on the importance of ancestor propitiation in Mapucheland, using quotations from Titiev as my outline.<sup>1</sup>

We are told by Titiev that, “As soon as a person dies word is sent out to his neighbors and to friends and relatives on other reservations” (p. 103), and that on these occasions, “a reservation functions as a group of kin” (p. 104). Although Titiev occasionally uses the word “patrilineal” in his monograph, he presents no detailed discussion of the structure of patrilineal descent groups.

This is a structure of Mapuche society which must be taken into account in order to understand the relationship between the living and the dead.

The Mapuche live on reservations usually composed of two or more small, localized, unrelated patrilineages, one of which is dominant and traces descent to and beyond the original chief of the reservation community. Lineage and reservation exogamy are practiced, each lineage (and segments of large lineages) marrying in a matrilateral system of fixed wife-giving and wife-receiving groups with respect to homologous units on other reservations.<sup>2</sup> The formation of any funeral congregation is contingent on the system of patrilineages and the structure of matrilateral marriage.

The feature of reservation exogamy has implications for the unit which "functions as a group of kin." There is no rule proscribing marriage between unrelated lineages on a single reservation, yet marriage does not occur along these lines. These lineages are unified politically and to a certain extent economically. I suggest that this "exogamic bond" places this relationship in a kinship framework which is symbolic of a high degree of solidarity.

Members of the reservation community are obliged to attend a comember's funeral (*awn*) on moral grounds, for absence denies corporate responsibility and might even suggest complicity in the supernatural machinations which are felt to have caused death. If we take a married couple as reference, there will be present at any funeral members of the man's lineage, who are the host group under the system of patrilocal residence and the core of most responsible participants. There will also be in attendance at least the household heads from nonrelated lineages co-resident on the reservation. From outside the reservation boundaries come members of the patrilineage of the in-married woman, who are a group having moral responsibility nearly equal to that of the husband's lineage. In addition to these groups, all members of the local ritual congregation (i.e., members of several nearby reservations who regularly participate in the *ñillatun* fertility rite) are invited to attend and are under considerable moral compulsion to do so. One or more units involved in the matrilateral marriage system are also part of this ritual congregation. Others among the ritual congregation constitute the "friends" mentioned by Titiev.<sup>3</sup> All who participate in the funeral rites demonstrate their good will and moral rectitude with respect to the deceased and his kinsmen. The composition of the funeral congregation changes with regard to the core of patri-kinsmen involved and with the specific connection to the deceased of affinal units.

Another important statement of Titiev's seems to need some clarification. He writes: "Two pairs of orators, known as *weupins*, representing the dead person's matrilineal (sic) and patrilineal connections, then deliver eulogies, during which they recite the life history of the departed one, call to mind his ancestry, dwell on his fine character . . ." (p. 104). The Mapuche do not have a duolineal descent system, as this statement might imply. What is indicated by the custom of *weupin* is that the deceased's patrilineage and that of the surviving spouse are represented formally at the funeral ceremony. The funeral ceremony is designed to get rid of the spirit of the dead person, as well as of any

other evil spirits which tend to lurk around the house, ceremonial field, and cemetery at this time. The recitation of the deceased's life history and the calling to mind of his ancestry are no more nor less than ancestral propitiation, since these words are not directed as much to the living audience as to ancestral spirits. These latter are supplicated to care for the deceased's spirit, and they are reminded that this is part of their obligation to the living. It is their responsibility to the living to protect the newly released spirit from falling prey to the forces of evil. The ancestors of the surviving spouse are invoked in like manner. This propitiation is a regular and systematic part of every funeral ceremony.

The funeral ceremony also involves highly stylized wailing and imprecation of the forces of evil which have caused death and which hover about to capture the newly released spirit. This may be considered in the context of another of Titiev's remarks that ". . . he who employs a sorcerer must be prepared, if the victim succumbs, to have one of his own kinsfolk die to even the score" (p. 109). The score is evened by sorcery in a system of vengeance-by-witchcraft, which is too broad to be discussed here. One thing that may be indicated, though, is that both shamans and sorcerers have ancestral familiars, and that both enter the supernatural world where they grapple with supernatural forces. For this reason, even the shaman is somewhat feared and sometimes labeled a sorcerer by out-groups. Both shaman and sorcerer are, in accordance with the rules of exogamy, in-married women who deal with spirits not patrilineally ancestral to core members of the residential kingroup.

Selecting only the matter of corporate responsibility for consideration, vengeance-by-witchcraft must be analyzed with respect to the responsibility of patrilineal descent groups. Any member of the murderer's group is fair game for revenge (i.e., is considered part of a group sharing responsibility for the action of any other member) in the system of supernatural reprisal—the functional equivalent of the formerly practiced blood feud. Since Titiev does not discuss the connection between this system and the importance of ancestral spirits, perhaps I might be allowed a cryptic remark which at least indicates what that connection amounts to. A potential victim of sorcery, as well as a person who has used magic to murder, is each afforded a margin of safety, provided his ancestors' spirits have been properly propitiated. All Mapuche evidence great concern with the ritual state of their ancestral spirits. A person attempting murder makes a special plea for them to protect him from reprisal. One's ancestors become offended by murder only if the life of a kinsman is in question—a good reason why Mapuche maintain that murder is never committed within a patrilineal group. This is another indication of the theme of corporate responsibility.

Returning to Titiev's account: "Once burial has taken place . . . the soul is supposed to go to the other world; it continues to play a part on earth as long as the deceased is actively remembered, especially if he is the subject of dreams. With the passage of time, when people no longer think, talk, or dream of a departed person, his death comes to be regarded as final" (p. 107). First of all, it

is not clear how the word "supposed" is used in this passage. Is it meant to suggest merely that this is something the Mapuche believe, a quaint notion, or does it really indicate the purpose of the funeral ceremony? Since Titiev's account merely approaches the threshold of analysis, some clarification seems in order.

The spirit is indeed supposed to go to the spirit world, the world of the dead, where it lives tranquilly. Spirits which linger after death are potentially dangerous—but not bad in themselves—because they may be captured by sorcerers and put to evil practices. There are several designations for the "essence" of the deceased which indicate differences in ritual state, alluded to by Titiev as a belief in "multiple souls." While the matter is simple enough, it requires discussion which would be digressive to the purpose of this note. A simple dichotomy, however, may be seen between "ancestral spirits" and "ghosts," the former benevolent and the latter demonic forms converted from the ineffable essence of spiritual beings. The funeral ceremony is geared to the safe passage of ancestral spirits to the other world and to the prevention of their capture and conversion into forces of evil. Another indication that the funeral rituals are largely mechanical devices to insure the departure of spirits is that great emotion at the loss of a kinsman is shown at the wake rather than at the burial service, by which time remorse is controlled to the end that the machinery of propitiation functions smoothly.

Thus, it is obvious that around the time of funeral activities the spirit is in danger of being captured and that this is of great concern to the living. It is impossible to agree with Titiev that "with the passage of time" the danger of spirits falling into the hands of sorcerers is lessened, which I take as part of the implication of his remarks. There is never any question, really, about the finality of the death of the body, as suggested by Titiev. But it is just the forgetfulness implied in the phrase "with the passage of time" which is a major pitfall in the Mapuche system of religious morality. The unpropitiated spirit *returns* to earth to haunt the dwelling of responsible kinsmen for the purpose, not of doing them harm, but of reminding them of their obligation. Spirits are good or, at least, neutral. They do no harm by themselves. They are not considered either desirous or capable of doing evil. When they are captured, a qualitative change takes place in them and they become ghosts of one sort or another, no longer considered as ancestors. To understand the full operation of this system it would be necessary to consider the marriage system in some detail, which I cannot do here. I might point out, however, that one is most likely to neglect the ancestors acquired through affinal ties. I can only indicate that these matrilineal ancestors loom large in sorcery, that in-married women are most suspect of practicing sorcery within the residential group, and that both shamans and sorcerers have as their most powerful familiars spirits from wife-giving groups.

There is one instance, not mentioned by Titiev although contained in one or more of Hilger's verbatim accounts, in which ancestral spirits may return to earth with impunity and without endangering the living. These, however, are

special ancestors, and even they dare not walk alone. Ancestral chiefs, who are in all cases heads of dominant lineages, may and do return to earth frequently to watch over the affairs of the living. This is part of their responsibility, symbolized in the great *ñillatun* ceremony. These chiefs are accompanied by sons of certain gods, which indicates the hierarchical ordering of ancestors' spirits, special ancestors' spirits, sons of the gods, the pantheon and, at the apogee of this structure, *ñenechen*, the Supreme Being.

Titiev writes further about the spirits of the dead: "If they are displeased by their funeral services, or if they are manipulated by witches, spirits can wreak harm on mankind, but no systematic rites of propitiation are carried out" (p. 108). According to my experience with the Mapuche, this statement does not make the proper connection between ancestral "displeasure" and the control of ghosts by forces of evil. There is no indication in this statement of a difference in good and evil qualities, and a false note is struck in implying that ancestral displeasure is aroused by funeral services. Displeasure, if it may indeed be called that, comes later and involves the return of the spirit. If the spirit is captured at the time of the funeral, it means that there has been a mechanical breakdown in the ceremony and that the sorcerer-witch (*kalku*), the embodiment of evil, has been successful. More than being displeased, the recently departed spirit is utterly helpless. As to Titiev's denial that "systematic rites of propitiation" are performed, I must dismiss it. What I have said so far indicates that such rites are part of the funeral service. They are also part of the *ñillatun*, especially on the first day of that ceremony which is not open to all comers. On all ritual occasions, for that matter, ancestral spirits are systematically engaged by the living. In addition, libations of wine and *chicha* are poured to ancestral spirits whenever they are available for consumption.

Before concluding this set of comments, I would like to quote from a general statement of Titiev's about the *ñillatun* ceremony, since it is indicative of his conception of Mapuche society and his position with regard to the structure of Mapuche morality: "Woven together into the great composite of *ñillatun* rites are elements of *machi* lore [i.e., shamanism], assembly customs, hockey games, curing and divination practices, and numerous other religious features . . . designed to give thanks for favors received or to ask help in times of stress. This greatest of Araucanian ceremonies is hard to define in precise terms. Above all one cannot determine the names or characteristics of the deities or spirits to whom the prayers are addressed" (p. 128). This is not the place to catalogue or characterize the pantheon of gods and goddesses headed, in a sense, by *ñenechen*. I merely mention that some of these might be considered local deities, or at least those receiving local emphasis, although known to all or most Mapuche. Rather than the cultural "shreds and patches" suggested by Titiev there is actually a high degree of cultural uniformity. The differences which exist seem more profitably studied sociologically rather than culturally. Ancestors' spirits vary with the groups concerned, as does emphasis placed upon one or another set of gods. But, in an over-all sense, there is little or no differ-



ence in either the structure or the function of the ñillatun or awn in any part of Mapucheland.<sup>4</sup>

I conclude with a consideration of one of Titiev's more insightful remarks: "Apparently, the Araucanians believe in a sort of dualism in which good powers that sustain society oppose evil forces that disrupt the social order" (p. 114). When Titiev noted this dualism he struck a major chord in the scale of Mapuche values. The eternal struggle between good and evil is a theme given daily expression in Mapucheland, and one which continually engages ethereal ancestors and their descendants. It is a theme recounted around the seemingly eternal hearthfire. It is dramatized in the periodic assemblages of the ritual congregation. It is the be-all and the end-all of the thought and activity of shamans and sorcerers. It is signalized by the cry of the nightbird and by the wavering luminosity of *anchimallen* and the black-shrouded, saber-toothed *witranañwe* who roam the earth after dark. One is instructed in the ways of avoiding evil. But transgression of the line between good and evil is always a danger and is fostered by the activities of the sorcerer.

The Mapuche world revolves on the moral axis between the living and the dead. The spirits of the dead either enjoy the eternal shadow world where they exist comfortably or, if improperly cared for (i.e., not "actively remembered"), return to lurk among the living. If captured by witches, these spirits become polluted, malevolent forces which are doomed to interminable nocturnal wanderings either as ugly nonhuman forms or as distorted wraiths of their former selves. Thus, they pose a constant threat to the living—not only to their former kinsmen, but to all Mapuche—as generalized forces of evil. The good life (symbolized by health, the reflection of morality) is guaranteed, at least on balance, by the offices of the shaman and the ritual priest (*ñillatufe*).

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#### NOTES

<sup>1</sup> Hilger worked with the Mapuche in 1945 and published her monograph in 1957. (See my review of Hilger, 1959.) Titiev worked with the Mapuche in 1948, publishing his monograph in 1951. My own work was done between November 1953 and January 1954, and my first monograph is now in the press. I cite Titiev's work in the foregoing pages because it is widely known and provides a convenient point of departure.

<sup>2</sup> I have recently completed and submitted for publication a short paper called "A Note on Mapuche Marriage" in which the alliance system is more fully explained.

<sup>3</sup> I have not mentioned the festive aspect present in all Mapuche ceremonies, and the simple desire of Mapuche to attend the eating and drinking feasts, because it is a consideration not germane to this paper. It has something to do with friendship, however, which, I might point out, always implies some sense of kinship.

<sup>4</sup> The reference is only to Chile, although Hilger's study suggests that the Argentine Mapuche are rather similar to those of Chile. I also confine my remarks to traditional ñillatun, not to those ceremonies in which shamans now participate. While shamanistic performance is an important structural modification in ñillatun, this is not the place to discuss it.

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## Letters to the Editor

### ON PSYCHOLOGICAL DEFINITIONS

Sir:

In his review of *A Comprehensive Dictionary of Psychological and Psychoanalytical Terms* Sidney Axelrad (1959) does not mention the many terms drawn from anthropology and sociology, nor does he critically evaluate the usefulness of our psychological definitions for the anthropologist—a criticism we should like to see. Instead he centers his attention on the “many errors” in our treatment of psychoanalytic terms, of which he cites three examples:

(1) “Psychoanalysis is defined without reference to resistance or defenses.” What we say is: “The doctrine is based on unconscious motivation, conflict, and symbolism.” Of course that is not the whole story; a dictionary definition limits itself to *basic* concepts. Now, however important the concepts of “resistance” and “defenses,” they are not basic but derivative concepts—they are corollaries to “unconscious conflict.” In their own place, however, “resistance” and “defense” are duly defined.

(2) “The ego is defined as conscious.” As it was by Freud! But we add: “Many later writers do not separate ego and id, speaking of id-ego.” Surely, Dr. Axelrad knows that the id is unconscious. But even those analysts who speak of a combined id-ego speak of reality-oriented and conscious functions as ego functions.

(3) “Defense mechanisms are not stated to be unconscious.” Under *defense* we write: “The defense is usually (if not always) fully unconscious.”

To use another lingo, the defense rests. We should be most happy if among the 13,000 terms defined there were no graver errors than these three.

But of course there are—a few that have already turned up are what the Little Flower used to call “beauts.” We therefore reiterate the plea we make in the Preface to be informed of errors. We shall be especially grateful to hear from readers of this journal of inadequacies in anthropology.

HORACE B. ENGLISH  
*Ohio State University*

#### REFERENCE CITED

AXELRAD, SIDNEY

1959 *Review of A comprehensive dictionary of psychological and psychoanalytical terms*, by Horace B. and Ava Champney English. *American Anthropologist* 61:548-50.

#### MORE ON PSYCHOLOGICAL DEFINITIONS: REJOINER TO ENGLISH

Sir:

I shall confine my rejoinder to Dr. English to the review that I wrote, not the one that he would have preferred that I write. I do not mean to quarrel over how many errors there are in the dictionary. The point is that a dictionary, which is used as a reference work for scholars, can afford a minimal number of errors, and certainly cannot afford significant errors of the kinds pointed out below:

(1) Resistance and defense are not derivative, but basic concepts for psychoanalysis. For proof, Dr. English might consult the corpus of Freud's works, from the very early *Project* and the letters to Fliess, to such later books as *Inhibition, Symptom and Anxiety*, and the *Outline of Psychoanalysis*. But Dr. English has not only omitted two basic concepts, he has included one which is derivative, symbolism, which is an outcome of, and thus secondary to *displacement*, which he does not mention.

(2) It might be wise to quote more fully the English's definition of the ego: "that aspect of the psyche which is conscious, and most in touch with external reality." Because this definition implies that the ego is wholly conscious, it errs. The standard psychoanalytic usage of the term includes unconscious aspects, as well as conscious ones. I refer Dr. English to A. Freud's *Ego and the Mechanisms of Defense* and to S. Freud's *Ego and the Id* for further clarification. The point is not whether I know that the id is unconscious, but whether Dr. English is aware of the unconscious functions of the ego, and if so, why he has failed to include them in his definition of the ego. Surely Dr. English must be aware that the fact that the id is unconscious does not mean that the ego is wholly conscious. Dr. English has confused structure with system. I must plead ignorance of the term id-ego. Would Dr. English indicate which analysts use this term, and where they do so?

(3) Dr. English seems to be confused about which terms I refer to. I commented on his definition of *defense mechanism*, not of defense. He refers to what he has written about defense, a definition of another term. It is true that this is a matter only of a word, but then, we are dealing with a dictionary. And a more correct statement about defenses does not justify an erroneous definition of defense mechanism, and his is wrong since it omits the fact that the defense mechanisms are unconscious.

SIDNEY AXELRAD  
Queens College

#### SCIENTIFIC METHOD IN MONOGRAPHS ON RELIGION

Sir:

In the AMERICAN ANTHROPOLOGIST (61:1147) D. B. Shimkin has reviewed my book, *Die primitiven Seelenvorstellungen der nordeurasischen Völker, Eine religionsethnographische und religionsphänomenologische Untersuchung*. He arrives at a rather peculiar final judgment of my treatise (in striking contrast to the positive opinions expressed in the corresponding European journals), which, according to him is "a theological brief rather than a scientific treatise." At the same time, however, he is obliged to acknowledge that "Paulson unquestionably brings together much factual evidence of 'soul dualism' in northern Eurasia." I should here like to ask whether the collection and arrangement of "much factual evidence" in support of one's main thesis or theory is not "scientific"? Or is it unscientific if one "also engages in extensive interpretations to make obscure and heterogeneous observations fit theory"? Has Mr. Shimkin himself never met, in the course of his own ethnological research, with "obscure and heterogeneous observations" that he has been obliged to analyze and interpret? I cannot help having discovered in the course of my investigations many "secondary origins" that, in their nature, were a help to me to "explain inconvenient contradictions" in the notions of the soul discussed by me. But it is not quite correct that, as Shimkin asserts, "the author is especially eager to deny autochthony to monistic conceptions." In many places (Paulson pp. 211 ff., 245 ff., 315 ff.) I have conceded such an origin to the monistic conceptions of the soul which I have found in my material. That "the general

burden of the study is a polemic against the views of W. Schmidt and A. F. Anisimov" (i.e., the monotheistic and materialistic theories) is an impossible claim, as I have discussed these views on only 6 pages (pp. 214-19) in a dissertation of over 400 pages!

That "soul dualism rests upon an elemental concept (*Elementargedanke*) of mankind" I have at the end of my treatise (p. 372) ventured to assert, not on the strength of my own results in northern Eurasia alone, but as a conspectus of painstaking and very detailed investigations in different parts of the world by many researchers (e.g., Ankermann for Africa, Hultkrantz for North America, and Arbman for various parts of the world; cf. also Lowie 1948:348f.; Boas 1940:596-607). On p. 371 I have not with a single word mentioned western Asia as the region in which soul monism originated, as Shimkin asserts. What is there spoken of is the role played by the great world religions (Buddhism, Christianity, Islam) in the distribution of soul monism. On the basis of a large number of works by other investigators I have tried to locate the origin of soul monism in the old Eurasian high cultures, from India to the Mediterranean countries, western Asia of course included. As regards North America, Hultkrantz has in his work (1953) stressed the role played by Central America.

Finally, a few words concerning my "two other weaknesses," according to Shimkin. I find that "classifications of ideas on human souls" are "meaningful" also in a context like that described by Anisimov for the western Tungus (Evenki) where he speaks of "concepts identifying parts and wholes, individuals and clans or species, and the actual, the fetishistic and the mythical" (Shimkin). From the same Tungus the same investigator has in another connection described also very sharp classifications of ideas of the human souls (Anisimov 1958:56 ff.). Shimkin is determined to "see no unity in the particular grouping of peoples examined" by me and finds it "culture-historical nonsense" to treat all peoples belonging geographically to northern Eurasia in one connection. But my work is not a culture-historical study, it is a study in the phenomenology of religion, as I have made sufficiently clear (e.g., pp. 11 ff., 212ff.). In my introduction (pp. 14 ff.) I have also referred the reader to earlier works (inter alia, by the great Finnish researcher Uno Harva [Holmberg], Gustav Ränk a.o.) which, like my own treatise, move precisely within the above mentioned geographical boundaries.

I am sorry, but I cannot accept Mr. Shimkin's criticism of my work.

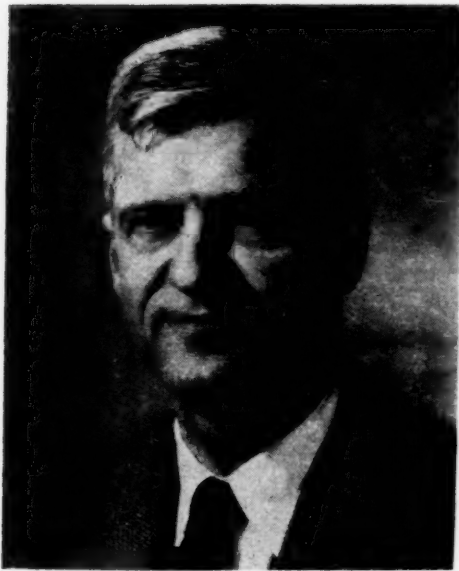
IVAR PAULSON  
*University of Stockholm*

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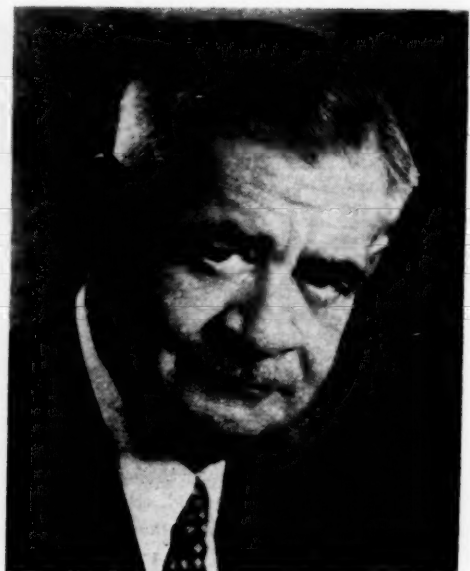
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LESLIE SPIER




SHERWOOD L. WASHBURN



SAMUEL K. LOTHROP

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## Viking Awards 1960

FOR the 15th—and final—time since 1946, the Wenner Gren Foundation for Anthropological Research on March 3, 1960, awarded its Viking Fund Medals for “outstanding achievement in anthropology.” The annual awards went to Drs. Leslie Spier, Sherwood L. Washburn, and Samuel K. Lothrop.

Leslie Spier, the 1960 Viking Fund Medalist of the American Anthropological Association, has given long and distinguished service to anthropology. He was one of the middle period group of Boas students, taking his doctorate at Columbia in 1920. After a period of time spent on the staff of the American Museum of Natural History, he taught at the University of Washington, at the University of Oklahoma, at Yale University, and finally at the University of New Mexico. He has complemented his teaching and museum activities by the editorial services rendered his profession, as editor of the *AMERICAN ANTHROPOLOGIST* and the *Yale University Publications in Anthropology*, and as founder and editor of the *General Series in Anthropology*, and later one of our major journals, *The Southwestern Journal of Anthropology*. He was president of the American Anthropological Association in 1943.

The Association cited him as follows: “He has ranged widely in his scholarly activities; his early archeological studies in New Jersey produced significant results, and later, when at the University of Washington, he carried on researches in physical anthropology. He is best known, however, for his studies in the field of American Indian ethnology. His investigations have been marked by a meticulous regard for methodological considerations and elegance in utilizing the instruments of his science. Though he has worked in the ethnographic tradition, he has been alert to the theoretical implications of his data, and was one of the earliest American students to recognize the importance of the study of the results of contact between peoples in as precise terms as possible, not only between Indians and those of European tradition, but also as concerns contact between groups of Indians themselves. One of his most notable contributions is to be found in his summary of the extensive study, by members of the staff of the American Museum of Natural History, of the Sun Dance of the

Plains Indians. More than any other student, he made us cognizant of the limitations of diffusionism, and pointed the way toward what later became field studies of cultural change, comprising the whole area of research that now comes under studies of acculturation. As creative scholar, as writer, as editor, as teacher, his contributions are of first magnitude. In honoring Spier for these, the American Anthropological Association once more honors itself."

Sherwood L. Washburn was chosen as the recipient of the 1960 Viking Fund Medal and Award in Physical Anthropology. The selection was made by a seven-man committee of the American Association of Physical Anthropologists, all of whom are themselves previous recipients of the same award.

Dr. Washburn received his B.A. degree from Harvard in 1935 and his Ph.D. in anthropology there in 1940. His graduate work was done in the department which was then directed by the late Professor Earnest A. Hooton. Dr. Washburn served, first as an instructor and then later as an assistant professor of anatomy, at the College of Physicians and Surgeons of Columbia University during the period from 1939 to 1947. He then joined the faculty of the University of Chicago as associate professor in the Department of Anthropology, where he was subsequently promoted to a full professorship. Since 1958 he has been professor of anthropology at the University of California at Berkeley.

He ably served the profession as president of the American Association of Physical Anthropologists (1951-52), and is now president-elect of the American Anthropological Association. He has also been editor of the *American Journal of Physical Anthropology* (1955-57) and of the *Viking Fund Publications in Anthropology* (1956-59).

Dr. Washburn was selected as recipient of the 1960 Viking Fund Award in recognition of his important contributions to the field of physical anthropology. The citation by the award committee says: "Especially noteworthy among his contributions are his numerous studies of the development of the adult skeletons of man and various animals, his pioneering attempts to modify experimentally the adult form of various skeletal parts, and his well documented field observations on the social behavior of baboons, which he undertook in an attempt to obtain additional information which could be useful in further elucidation on the evolution of human behavior—a topic which is now Dr. Washburn's principal research interest."

Samuel K. Lothrop was cited by the Society for American Archaeology. He received his M.A. from Harvard in 1915 and his Ph.D. from the same institution in 1921. He is now the Curator of Andean Archaeology at the Peabody Museum of Harvard University. A member of many scientific societies, he has also received the Loubat Prize from Columbia University, the Alfred Vincent Kidder Medal from the Society for American Archaeology, and the Huxley Memorial Medal from the Royal Anthropological Institute. His field research has been on behalf of the Robert Peabody Foundation (Andover), Peabody Museum (Harvard), Carnegie Institute of Washington, Museum of the Amer-



ican Indian, Heye Foundation, Museum of Archaeology and Ethnology (Cambridge, England), and Museo de la Plata.

The Society for American Archaeology recommended him for the award with these words: "Lothrop has probably published more major works of highest quality than any other Americanist. They range from Guatemala to Argentina and include valuable ethnological papers dealing with Guatemala and Tierra del Fuego. In these works, he has set high standards of ethnohistorical research and of scholarly presentation. His monograph on Coclé (Panama), in particular, stands out as a monument of careful excavation and accurate reporting.

"The full value of Lothrop's research is appreciated only by those who have followed him in the various areas of his activities. His work in Mesoamerica is basic to a proper understanding of the cultures of the area and has laid the groundwork for much of the recent change in the concept of the Maya. His reports on Costa Rica and Panama provide the essential foundation for all investigations in Central America and his monograph on the metal work from the *cenote* at Chichén Itzá is an outstanding publication. In his broad knowledge of Latin American studies, he has no peer."

Next year, only one outstanding anthropologist will be chosen to receive the Medal. The Medalist is to be elected by the international Associates of *Current Anthropology*, the new scientific journal providing communication among anthropologists of six continents. Previous selection was by the three major American anthropological societies.

## Book Reviews

### GENERAL AND ETHNOLOGY

*Culture in History: Essays in Honor of Paul Radin.* STANLEY DIAMOND (Ed.) Foreword by ABRAM LEON SACHAR. (Published for Brandeis University.) New York: Columbia University Press, 1960. xxiv, 1014 pp., bibliography of writings by Paul Radin, chapter references, figures, frontispiece, illustrations, index, maps, tables. \$15.00.

Reviewed by ROBBINS BURLING, *University of Pennsylvania*

This massive and diverse collection which honors Paul Radin has received contributions from 54 authors, including Cora Du Bois who provides a warm appreciation of the man, and Richard Werbner who arranged the bibliography of his works. The papers range topically from the examination of Marxian texts to linguistic classification; geographically over the entire world; and temporally from preclassical Greece to modern times. No man, except perhaps Radin himself, could hope to embrace all these fields of interest, and I can do no more in this review than select a few papers which appealed to me or which I think may be of interest to other anthropologists and try to convey a few of the impressions that emerge after immersion in its pages.

I suppose that *Culture in History* is as good a title for this book as any other. Several authors do indeed offer histories of various cultures, or examine certain aspects of culture within a limited time and place. John Murra, for instance, argues cogently that agriculture in the Inca area was characterized by two sets of crops and correspondingly different agricultural and ritual systems. Maize, a state crop, was grown with irrigation and surrounded by elaborate state ritual, while tubers were the staple subsistence foods of most of the ordinary people and had a different set of associated rituals. The earliest chroniclers generally ignored the more humble of these traditions and so have left a one-sided view of an area with two very different ways of life. Walter Goldschmidt gives a translation of a fascinating speech by an African official who had recently returned from England as a demonstration of the manner in which ideas and impressions of the Western world are interpreted to people of a remote region of Africa. John Phelan shows how Mexican nationalists have seized upon the Aztec past as a symbol of their differences from Europe. Wilson D. Wallis argues that the mythology, cosmology, riddles, rituals, and dance forms of southeast Asia, Indonesia, and Polynesia on the one hand, and those of Greece, Rome, and the Indo-Iranian area on the other are similar enough to suggest historical connections. Nancy Lurie and James Griffin examine the protohistory and prehistory of Radin's own Winnebago. These and many other papers attack problems which might be called "culture in history," and many of them are interesting and well done in their specialized ways.

However, if I were forced to designate a focus that stands out from the diversity of this book, it would not be "culture in history," but rather an interest in what many people would call "cognitive processes," but for which, happily, many of these authors use such old fashioned words as "mind" and "thinking." The ability of the human mind to produce symbols and to organize symbols into myths and into coherent world views was a subject of prolonged interest to Radin, and many of the authors deal with one or another of these topics. One way to approach the question is to examine the terminological distinctions which people make in describing some aspect of their world, assuming

that terminological distinctions parallel behavioral and conceptual distinctions. It is apparently something of this sort that Kluckhohn attempted with his survey of a miscellaneous selection of Navaho "categories." He points out that the Navaho distinguish between "behavior," "myth," and "beings"; between "evil magic" and "good ceremonialism"; between "poison witchcraft," "spells," "pellet witchcraft," and "frenzy witchcraft"; between "hand-trembling," "star gazing," "listening," and "datura divination"; between "male" or "coarse and rough," and "female" or "fine, weak and gentle"; between various kinds of winds; between particles, nouns, and verbs, and so on. I see no criteria by which Kluckhohn selects the particular categories to include in his treatment, and when he has finished he seems to have done no more than to show that the Navaho do make certain terminological distinctions. In his summary he states that several particularly important criteria are used by the Navaho in distinguishing categories. Among these are ". . . descriptions according to perceptible or symbolic similar characteristics. This principle of classification appeared to greater or lesser extent in every sphere examined" (p. 93). If I am penetrating the dubious grammar of this sentence, it seems to mean that Navaho may recognize as different those objects which appear different. *Mirabile dictu.*

In a much more focused article, which has a partially similar point of view, Hallowell explores the world view of the Ojibwa. He points out in effect that the verbal and behavioral distinctions which the Ojibwa make in their environment are not the same as ours. The grammatical distinction between animate and inanimate does not correspond to our distinction between living and nonliving, for such things as "stones" are classed as animate in the Ojibwa language, and this is reasonable since stones are felt to have certain properties of animation. Stones have been observed to move and even to speak. On the other hand, the distinctions which we make between humans and others, or between natural and supernatural, are not clearly drawn by the Ojibwa. Among other things these distinctions are blurred by the metamorphosis from animal to human form which Ojibwa know to be entirely possible. Our distinction between personal and impersonal forces is not found, and, in fact, the Ojibwa tend to view events in personalistic terms and to shun impersonal causes. Sickness, for instance, is seen as being caused by human actions. From such considerations Hallowell constructs a picture of the world view of the Ojibwa as built around their understanding of the nature of "persons."

Several authors deal with some aspect of mythology either in Western or in non-Western traditions. Lévi-Strauss analyzes four Winnebago myths which Radin had published, and he attempts to show that all four have an underlying unity, though they differ widely in content, style, and structure. Earl Count, in a long and involved article, attempts to tie the myth-making ability of man to his emerging biological capacities. Melville Jacobs categorizes humorous situations found in Clackamas myths, including both those types of humor which are probably panhuman (slapstick, skillful mimicry, incongruous language errors, etc.), and others more restricted in their distribution (anatomical reference, attitudes toward pomposity and vanity, pleasure in smart fakery or trickery, etc.). Jacobs is certainly right in asserting that humor deserves more study by anthropologists.

Other aspects of man's intellect are examined also. Redfield, in the initial paper in the book, considers the position of a thinker or an intellectual in a primitive society, and in case anyone should want a survey of the long argument over whether monotheism existed among primitive people before they fell under the influence of Western traditions, David Bidney provides it.

Radin contributed not only to the study of the expressive use of language as in

myths, but to the structural aspects of languages as well, and several authors in the book deal with linguistic problems. Most of these take up rather specialized matters, but Eric Lenneberg, in a solid though hardly ground-breaking article, notes the considerable extent to which our linguistic ability can be said to rest upon our innate biological characteristics. Lenneberg points out that we undoubtedly have a biological potential for at least three characteristics of speech which are universal: phonematization, concatenation or the stringing together of morphemes and words into complex sequences, and grammatization. I would have added, as Lenneberg did not, that the most remarkable of all characteristics of language, the ability, or rather the imperative, which forces men to assign meanings to sequences of sounds, is itself part of our biological equipment. I am not sure whom Lenneberg is trying to convince, but if anyone *does* need convincing that our linguistic abilities rest upon the organization of our biology, then here is the place to look.

Other papers are so diverse as to defy categorization. Greenberg does a most impressive job of analyzing the prosodic features (rhyme, alliteration, syllable count, quantity, stress, and tone) which occur in certain types of North African poetry, and of discussing the distribution of these features in North Africa and near North Africa. Swadesh carries some of his recent studies to their logical conclusion and presents a collection of data purporting to show similarities between the languages of the two hemispheres. This is crowned by a list of about two dozen sets of lexemes taken from Indo-European, Semitic, Malayo-Polynesian, and occasionally Basque, Dravidian, Munda, Uralic, and Sino-Tibetan, and all sorts of American Indian languages which are supposed to suggest a single common origin. He even suggests reconstructions for these items. Whew!

Morton Fried, in a frankly speculative article (more frank and less speculative than Swadesh's), suggests an evolutionary sequence of political and economic forms from simple "egalitarian societies" in which no leader stands out as having distinctively more prestige than others, to "rank" societies in which there is a prestigious individual who plays a redistributive role in the economy, to "stratified" societies in which for the first time some individuals have a privileged use of political and economic power, and finally the "state" society of the sort that has emerged only a few times in history. He makes some suggestions about the possible mechanisms that may have led from one to another of these increasingly complex political and economic levels.

Finally, I found the article by Huntington Cairns to be completely intriguing. He discusses the relevance of the several possible sources of knowledge (authority, mysticism, rationalism, empiricism, pragmatism, and scepticism) to our understanding and knowledge of legal data and theory. He shows that in practice the legal theorist must rely on others (authority), that he must start somewhere with unprovable postulates (intuition or, in a broad sense, mysticism), that he would like to have his legal premises set up carefully enough so that they are logically compatible and even capable of giving rise to reasonable deductions (rationalism), that he must consider his own observations of the world (empiricism), that the consequences of beliefs and actions are significant in propounding legal theory (pragmatism), and finally that a healthy dose of doubt is helpful in considering legal questions (scepticism). In short, all the sources of knowledge have been used and must be used in developing an understanding of law, and even so, we must realize that in the final analysis, "all human knowledge is uncertain, inexact, and partial." What goes for legal theory certainly goes for all the subjects which anthropologists study, and a little firm understanding of the ultimate sources of our knowledge would not do any harm to a good many anthropologists.

There is much more in this book: J. Alden Mason on the role of the ethnological

museum; Evans-Pritchard with a bibliographic survey of ethnographic work done in the Sudan; Mandelbaum on caste in India, and so on and on. In sum, this book contains something for everybody, and everybody will find some things in it which are well beyond his ken. I doubt if many of the papers are destined to become classics, but a high proportion are solid and competent contributions to their own specialized areas. Above all, it is the very diversity of this book which is both its chief failing and its triumph. It makes it impossible really to read this book or to review it sensibly, but at the same time it is a tribute to the man to whom it is dedicated. Only the breadth of Radin's interest really gives it unity. Tragically, the one man who might have appreciated all of the articles died before the book appeared.

*Man, Mind and Land: A Theory of Resource Use.* WALTER FIREY. Glencoe, Illinois: The Free Press, 1960. 256 pp., 7 figures, index. \$6.00.

Reviewed by RICHARD N. ADAMS, *University of California, Berkeley and Michigan State University*

This is a general theory of resource use drawing upon variables derived from ecology, economics, social psychology, sociology, and cultural anthropology. The book is systematic, theoretical, conceptual, and uses four sets of empirical material for testing and illustrative purposes. The presentation is methodologically precise and presents the major hypotheses in logical notations as well as English; the style is consequently rigid and laborious.

Although the intended audience of the volume comprises people specifically concerned with the development and conservation of natural resources, it can be recommended to anthropologists concerned with the ecological aspects of human society and to those involved in applied work. Firey sets forth the concept of the *resource process* as "... an event which recurs in time and which involves somewhat the same combination of human and biophysical factors" (p. 13). He then distinguishes between two kinds of systems of resources processes: the *complex*, a system of processes showing stability within a set of external conditions, and the *conger*, an evolving system of resource processes showing no such stability. He later suggests that changes in natural resource usages may be seen as complexes that are subjected to extreme changing external conditions and thereby become congeries; or they are the latter and are inherently unstable and therefore changing.

Firey holds that one cannot expect a given resource system to be simultaneously optimal in terms of ecological, anthropogenic, climax (i.e., best for the environment); and optimal for the value system of a population (i.e., have only valued elements present); and optimal for maximal gainfulness in terms of the rational allocation of productive factors in a free, competitive market economy. He then deals with two variables: whether a given practice is gainful (in the economic sense) and likely (i.e., whether one expects others to do it). He holds that a resource complex will have gainful-and-likely elements, and nongainful-and-likely elements, but that the entrance of gainful-and-nonlikely elements is the way that the complex becomes a conger. Put another way, a stable system of resource use will become unstable if the external conditions change such that new gainful behaviors are taken in spite of their uncommonness in the society. However, in the penultimate chapter Firey concludes that it is neither gain nor commonness that leads to the conservation of resources, but rather "... the ambivalent commitment of resource uses to standards of others' as well as private gainfulness. . . ." Through a knot of rigidly defined concepts, the reader is brought to the idea that if a set of resource processes is to change, they must do so through a commitment on the

part of the users to the needs of others. In the final chapter, the issues of obtaining the consent of the user is treated differentially as it appears in the development and conservation of resources. If one is interested in the former, the aspect of private gain should be propagandized; if in the latter, then the aspect of security in personal relationships must be emphasized to "... enlist the willing conformity of ... [the] subjects." Development leads to resource congeries, and conservation to the maintenance of resource complexes.

Firey's propositions are not as new as is the particular conjunction he brings to them. He works from a familiar equilibrium systematizer's framework and concludes that if a person is to work for the good of others, he must do so at the expense of private, personal gain. For the present reviewer, these latter conclusions are by no means as stimulating as is the early part of this work where he provides some provocative concepts for the handling of habitat and societal factors within a single framework.

The regular conversion of statements into logical notational form has much to recommend it from the point of view of precision; but the book would be considerably easier to use if these notations were handled in parallel footnotes or held aside and treated as a set of logical statements in an appendix. Firey would do his readers a service if he would issue a pony.

*The Image of the City.* KEVIN LYNCH. Cambridge: The Technology Press and Harvard University Press, 1960. vii, 194 pp., bibliography, 62 figures, index. \$5.50.

*Reviewed by* EDMUND LEACH, *Center for Advanced Study in the Behavioral Sciences*

Towns of a sort have existed for at least eight thousand years, and from the beginning the town has been the focus of all that is most sophisticated in human social life. But the sociology of great cities is something quite different from the sociology of towns. The modern metropolis—the vast unbounded conurbation that is London or New York or Los Angeles—is a very recent phenomenon and all-pervasive. It is clear that the city, in this new sense, is rapidly replacing all earlier forms of residential grouping. It is already the case that in large areas of Western Europe and North America—and for that matter Australia—80 percent of the population live permanently in cities and have no acquaintance with the open countryside except as an area of occasional recreation. There is every probability that within the next two generations we may reach a state of affairs in which the great majority of the world's population will grow up to think of the Mammoth City as the normal human environment. The implications of this development are very great but, as yet, they have scarcely been examined. What kind of a jungle is this new man-made human habitat? How do we find our way about in it? What features of the concrete landscape do we pick out as friendly and hostile? In a desert of stone and tarmac, how do we delimit our home? The author of this book, who is an architect and city planner, and Co-director of the Joint Center for Urban Studies of the M.I.T. and Harvard University, has tried to answer such questions by way of a direct sociological and anthropological enquiry. Along with a number of research associates he conducted extensive interviews in Boston, Jersey City, and Los Angeles with the object of discovering how the ordinary unsophisticated inhabitant of such places perceives his environment. Does he feel himself to be in a trackless wilderness or in a tidy garden? Are his surroundings congenial or oppressive? What features lead to comfort or discomfort? Do the grandiose conceptions of city planners bear any relation to the feelings and aspirations of the natives?

The author's hope is that the city planner will do a better job if he first takes the trouble to discover how existing cities actually impinge upon the consciousness of their

inhabitants, but there are other angles to the story. For example, just where does the professional anthropologist stand in all this? No doubt the hunters and collectors, the pastoralists, and the agriculturalists of this world are all heading for extinction; does that mean the end of anthropology? A book like this suggests that the culture of the modern city presents problems which are quite as "anthropological" and quite as fascinating as the kinship systems of Australian aborigines. Those who find Kevin Lynch's ideas as stimulating as I did will get a lot more on the same theme from *The Future Metropolis* which is the winter 1961 issue of *Daedalus*.

*Two Cities of Latin America: A Comparative Description of Social Classes.* ANDREW HUNTER WHITEFORD. (Logan Museum Publications in Anthropology, Number 9.) Beloit, Wisconsin: The Logan Museum of Anthropology, Beloit College, 1960. viii, 101 pp., bibliography, check list, 2 diagrams, 44 figures, 3 maps. n.p.

Reviewed by CHARLES J. ERASMUS, *University of North Carolina*

This is an important book which marks a new direction in anthropological field work: the study of social classes in the preindustrial and industrializing cities of underdeveloped countries.

Whiteford compares social classes in Popayán, a Colombian city of 33,000 people, with Querétaro, a Mexican city of about 50,000. Fifteen months were spent in Popayán between 1949 and 1952; during 1958 four months were spent with a team of students in Querétaro. Popayán was selected for study because it was a traditional community little affected by industrialization. Although an attempt was made to pick a similar city in Mexico, the differences between the communities were pronounced and interesting.

A separate work on Popayán is in process, so the present book focuses mainly on Querétaro. Following the brief orientation chapters on the two cities and on the objectives and methods of research, the upper, middle, and lower classes are described for Querétaro in separate chapters, each of which ends in summary comparisons with Popayán. The last two chapters, which discuss such topics as mobility, inter-class relationships, and class structure, are based on comparisons between the two communities.

Popayán proved to be much more tradition-dominated than Querétaro. In contrast, change was the dominant theme in Querétaro. Here "there was not sufficient general consensus in regard to class memberships and class divisions to give them any operational reality," so the tripartite classification was "a construct of the research." The Mexican Revolution and recent industrial changes had helped to foster a strong emphasis on equality and had resulted in a looser social framework. Mobility was greater, but there was also a suggestion of insecurity in the lower class.

The basic three class system—upper, middle, and lower—compared for both cities, is subdivided into upper and lower components at each level, giving six subclasses. In Querétaro the addition of a middle-middle results in seven subclasses.

Membership in the Popayán upper class depends upon lineage, and those families in the lower-upper are the less affluent members of the class. In Querétaro, membership in the upper class can be achieved through financial affluence; the wealthiest subclass is the lower-upper composed of the nouveau riche. Few of the dwindling "old" families composing the Querétaro upper-upper are actually as old as the upper class families of Popayán.

The substantial middle class encountered in both cases indicates that the populations of Latin American cities are not composed simply of an elite and a large lower class. Moreover, Whiteford points out that the middle class is not a recent phenomenon in either case and in Querétaro has waxed and waned over the years.

In the upper-middle of Querétaro are the professional men, in the middle-middle the white collar employees, and in the lower-middle the skilled workers and artisans. Some of the latter are more affluent than many persons of the middle-middle but are lacking in education and proper manners. Occupationally, the Popayán middle class is much the same as that in Querétaro, but in Querétaro the middle class is much more self-conscious than in Popayán and considers itself more progressive and substantial than the upper class. The middle class is a rising group in Querétaro, whereas in Popayán it marks the end of upward mobility. A man whose family background limits his ambitions leaves for a larger city. In Querétaro almost anyone can rise through financial affluence into the lower-upper; and since many in this category are not even aware of the upper-upper or do not recognize it, one can fairly say that a Queretano can go to the top of his status hierarchy. In Popayán, by contrast, the most mobile sector of the population is the lower-upper. These are the less affluent members of the local aristocracy, who may either do well financially and move into the upper-upper or slip further down.

In both cities the lower class is impoverished and works largely in manual labor. In Popayán the lower class people are very unobtrusive and seem to enjoy life more than the rest of society. Despite their meager opportunities, lower class Payaneses look ahead with as much, if not more optimism, than their equals in the more equalitarian society of Querétaro. There is no element in Popayán to match the aggressive upper-lower class of Querétaro. Upper-lower Querétaro men have little respect for anyone. They resent the superiority of some middle class people and are rough in their treatment of the lower-lowers.

Whiteford's excellent study suggests innumerable problems for further work. His comparison supports the view that mobility goes with industrialization, and security with a more rigid class structure. But some discontented people leave Popayán to seek their fortunes where opportunities are greater. Perhaps the industrialization of cities nearby acts as a safety-valve for static communities like Popayán. And, as Whiteford points out, resentments of the lower class came dramatically to the surface even in Popayán during the national disturbances of 1948. Perhaps the mobile situation in Querétaro with its offensive and obtrusive lower class and its much more self-conscious and invidious middle class is really "healthier" than Popayán. Maybe the lower classes of Popayán internalize a great deal of frustration and aggression in facing their relatively meager opportunities. Popayán may be a powder-keg situation despite its air of stability and contentment.

One also wonders how those cities to which ambitious Payaneses migrate compare with Querétaro. Under what conditions does industrialization alone create mobility and equalitarian standards in rapidly developing areas, without the land reforms peculiar to the Mexican case?

The fact that in Popayán the "three class division emerged quickly and clearly as the configuration accepted by the society," while in Querétaro it was a "construct of the research" based on "distinctions which . . . Queretanos actually make themselves whether or not they recognize them explicitly," is significant. It indicates that future studies of class in other countries should also attempt to measure the degree to which—and the conditions under which—class divisions have their greatest reality to the society's members. Such a continuum among cities might prove as interesting as the status continuums within them.

Future work might also combine studies of class and family structure. Perhaps in those situations which are most static, the matrifocal tendencies Raymond T. Smith correlates with lack of opportunities for the upward mobility of males (*The Negro*



*Family in British Guiana*, Great Britain, 1956) will be apparent in the lower classes of some cities. Also, the hypothesis of Gideon Sjoberg (*The Preindustrial City*, Glencoe, 1960) that the extended family is more common among the preindustrial elite than among the peasants and lower city classes could be further tested, and the disappearance or persistence of extended families at any level profitably compared with increasing social mobility.

This book by Andrew Whiteford is among those at the vanguard of a shifting emphasis from content to structure in anthropological studies of nontribal peoples. Even though Robert Redfield as early as the thirties denied any concern with the Spanish or Indian provenience of traits in his studies of Tepoztlán and Yucatán, he was certainly concerned with the difference in content between the center and the peripheries of Tepoztlán and between the city of Mérida and the peasant and Indian villages of the interior. How the villagers were structured into the larger social order of Yucatán through class relationships was not revealed, for the interest of the observers was focused upon losses and gains in content along a rural-urban, cultural continuum. Even George Foster, who was labeled a structuralist by Mintz because he found "folk" characteristics among the lower classes of Latin American cities (*American Anthropologist* 56:87-92), was really led to this comparison by an interest in the historical provenience of content (*American Anthropologist* 55:159-71). It was Gideon Sjoberg who as early as 1952 (*American Journal of Sociology* 58:231-39) classed peasantry with urban lower classes in his bifurcated feudal or intermediate society and thereby emphasized the structural class relationships between rural and urban peoples in preindustrial society.

Future work among peasantries and preindustrial or industrializing urban centers will undoubtedly give increasing emphasis to the structural class relationships within the urban centers and between townspeople and peasantry. Studies of content will probably be linked more and more with studies of class, as will studies of acculturation and personality, etc. Even the "typology" approach will probably move further from its spatial or culture-area origins and closer to a vertical or class-structure frame of reference. Differences between "open" and "corporate" peasant types, for example, might be sought more directly in comparisons of class structures. Within this new trend and shift of focus—to which Charles Wagley and his students have already contributed through their work in Brazil—Whiteford's comparative study will stand out as a pioneering and highly original effort, in large part because of its urban locus. This locus does not exclude its usefulness to future students of peasant societies, however, for they must become increasingly conscious of urban relationships. Whiteford's book should be read by all cultural anthropologists; those planning to work among peasants and city people should make it part of their field equipment.

*Shoshone-Bannock Subsistence and Society*. ROBERT F. and YOLANDA MURPHY. (Anthropological Records, Volume 16, Number 7.) Berkeley and Los Angeles: University of California Press, 1960. (Issued November 23, 1960.) pp. 293-338, bibliography, map. \$1.00.

Reviewed by OMER C. STEWART, *University of Colorado*

Dr. and Mrs. Murphy engaged in ethnographic and historic research from 1954 to 1957 to prepare Dr. Murphy to testify as an expert witness for the U.S. Department of Justice in the hearing of the Shoshone case before the Indian Claims Commission in Denver, August 29, 1957. The Murphys tried to examine all historic sources dealing

with the Shoshone territory of Idaho, Wyoming, and northern Utah, and did field work during the summer of 1954 at Wind River, Fort Hall, and Duck Valley reservations. The Nevada Shoshone were not revisited for ethnographic study because the Murphys thought all sources had been adequately evaluated and summarized in previously published materials, especially the Bureau of American Ethnology Bulletin 120.

On the map entitled "Shoshone—Bannock Subsistence Areas" the Murphys locate 18 "main winter camp areas" and with dotted lines mark the "principal migratory routes." No ethnographic boundaries are shown. The text suggests many more places which could have been selected as "winter camps."

The Murphys cite 138 sources dealing with the Shoshone. The earliest is the report of la Verendrye, 1741, who went west from the Mandan until frightened by reports of "Gens du Serpent" in the vicinity of the Black Hills. For some reason not explained, the Murphys are somewhat doubtful that the "Gens du Serpent" are really Snakes (i.e., Shoshones). They do not hesitate to equate the Snake Indians reported in Saskatchewan in about 1750 as Shoshone. Most sources are allowed to speak for themselves.

The Murphys document the confusion in early reports and some recent ethnographic accounts dealing with southwestern Idaho. The difficulty is compounded by many sources designating the Oregon Paiute as Snakes, (i.e., Shoshone), and by identifying the Idaho Paiute, (i.e., Bannock) as Shoshone. There is no question about the close kinship and linguistic similarity of the Bannock to the Nevada-Oregon Northern Paiute. The only question unanswered is that concerned with ancient homelands of the Bannock, who have lived with Shoshone in Idaho and Wyoming for more than a century and yet have remained distinct. They might have formerly owned and occupied the Boise and Wieser region of southwestern Idaho, in my opinion.

The Murphys found, as I have, that the Shoshone were basically a Great Basin people who expanded onto the northern Plains when they first received horses from the Spanish via the Ute of Colorado about 1700. Without guns, however, the Shoshone could not stay on the Plains and retreated west of the Rocky Mountains and south of the Bitterroot and Salmon Mountains, where they were raided by Blackfoot and Crow. Following Lewis and Clark, American fur trappers brought guns to the Shoshone so that they could hold their own against raiding Plains Indians. The Murphys did not mention that in 1855 the Shoshone under Washakie went to the Fort Laramie treaty-signing well armed and confident, yet allowed the Crow, Sioux, and Cheyenne to claim and to be reassigned territory west to the crest of the eastern range of the Rockies. The role of Chief Washakie as a wise negotiator who finally obtained a reservation in Crow territory on the eastern slope of the Wind River range of the Rockies is outlined by the Murphys.

The diversity of Shoshone subsistence patterns in Idaho and Wyoming is documented by the Murphys, but I think they failed to appreciate fully the manner in which horse ownership created a separate class of Shoshone who left it to their horseless relatives to carry on the old localized Great Basin hunting and gathering economy while they, the mounted individuals from many districts, joined together. The foot-Shoshone of Idaho and Wyoming, one group called Sheepeaters in the mountains of Yellowstone National Park, lived like their poor relatives in Nevada. The horse-Shoshone toured the whole area. The same Indians, in season, took salmon on the lower Snake River or the Lemhi; gathered camas in southern Idaho; hunted antelope in western Wyoming; and then, in large hunting war parties, killed bison in Montana or eastern Wyoming. With dried buffalo meat and hide tipis, the mounted Shoshone returned to winter with their low-class-gathering-stay-at-home kindred. I agree with the Murphys that the Shoshone were "essentially egalitarian" (p. 333) and that chieftaincy was earned by

attracting supporters who might change allegiance. I would add, however, that the mounted Shoshone formed a single, unified upper class.

The Murphys are to be thanked for making available so quickly the results of their research for claims cases.

*Apache, Navaho and Spaniard.* JACK D. FORBES. Norman: University of Oklahoma Press, 1960. xxvi, 304 pp., bibliography, footnotes, 8 illustrations, index, 4 maps. \$5.95.

*Reviewed by* CHARLES KAUT, *Southern Illinois University*

This study, written by a historian, is, by-and-large, a road map and timetable prepared for the use of "archeologists, anthropologists, ethnohistorians, and other persons interested in Southwestern Indian history." It represents an important attempt to trace "in the greatest possible detail" the year by year history of the Southern Athapascans, especially in terms of their relations to one another, other linguistic groups, and the Spanish Empire. The author has selected for documentation the events of the century-and-a-half period from 1540 to 1698. He follows the Spanish campaigns in the Southwest down almost every arroyo. He has located geographically Apache (and Pueblo) groups during this period in a highly specific fashion. His maps must be carefully matched with the text so that the names of the groups can be related to the documentation.

The story he tells is of the disturbing effects on the aboriginal intertribal relationships when the Spanish entered the Southwest and of the military rôle of the Apache in forming a "barrier to the northward expansion of the Spanish Empire." Further, he shows how Apache groups "served as a refuge" for other Indians fleeing from the Spanish, and as such a source for rebellion.

He quite properly points out that "many of the generalizations current in contemporary secondary sources are inaccurate." However, he does not specifically identify the sources inaccurate in their generalizations so that we could have his evaluation of them. Further, some of the inaccuracies he cites have long since been corrected or laid to rest. For instance, Dr. Forbes tells us that the conception of the Apache as essentially predatory Indians who were at war with the settled peoples of the region prior to the coming of the Spaniards" is shown by his "reconstruction" to be "spurious" and based on "false preconceptions and the inherited anti-Apache bias of Hispano- and Anglo-Southwesterners." My own experience with the anthropological "secondary sources" has not left me with this particular "spurious" conception, but rather, I must agree with his statement, that "the essential relationship existing between the Athapascans and the Pueblo Indians prior to Spanish interference was one of peace and commerce." However, I might modify this somewhat in that particular Apache groups seem to have had close relations with specific Pueblo groups, not so close with some, and looked upon others as potential raiding sources. However, it is probably a good thing that he reinforces the point and underscores the *true* nature of Apache political and economic relationships in the symbiotic network of the Southwest in the pre-Spanish period—that is, they served as important middlemen.

In conclusion, I have taken up neither his hypotheses concerning the prehistory of the Southwest nor the problem of "Unknown Athapascans" here because I do not believe the value of this work hinges upon the validity of the former, nor on the acceptability of his claims for membership of such groups as the Jumano in the Southern Athapaskan Club. It would be difficult for me to assess Dr. Forbes' research and analy-

sis. I think this must be done in future comparative ethnohistorical treatments much more comprehensive and exhaustive than any so far attempted. Certainly he has presented us, in this volume and in his other published work, an ordering of much valuable information which should be used as part of one's orientation for the study of social change and interaction in the Southwest.

*Ingalik Mental Culture.* CORNELIUS OSGOOD. (Yale University Publications in Anthropology No. 56.) New Haven: Department of Anthropology, Yale University, 1959. 195 pp., appendices, 10 figures. \$2.50.

*Reviewed by* EDMUND CARPENTER, Northridge, California

This third and final volume of Osgood's reconstruction of the life of the Anvik-Shaeluk deals with the "mental culture" of this branch of the Athabaskan-speaking Ingalik. The earlier volumes described "material culture" and "social culture," respectively. This three-fold division is not the traditional economy-society-religion one, but rather a very precise effort to clarify the nature of such data by correctly categorizing them. Thus, each volume is both an ethnographic report and a theoretical adventure.

Data often get overlaid by habit, becoming buried under the weight of familiar concepts and tradition. Sometimes an entirely fresh context is needed before their message shines clearly. The analytical method Osgood has chosen is not new, but he has applied it with unparalleled rigor. Material products, he notes, can be collected and behavior can be observed, but an informant's memories about them are "mental culture," even though it is hoped his reconstruction is accurate. Moreover, Osgood argues, an informant's ideas *about* objects and behavior should be distinguished from these objects and practices; the expression of ideas is perceptible, but the ideas themselves are not, e.g., the sound of speech can be perceived but not the ideational meanings or concepts it labels; and finally, "a clear distinction between non-empirical ideas *about* things and empirical ideas *of* things is essential," e.g., the idea of the salmon and the idea "that salmon will run in larger numbers because the first to be caught is ceremonially treated."

These and other distinctions bring his Ingalik data into sharp focus. Osgood has a superb command of words; he is at all times clear, direct, and coldly analytical. His writings leave one with the impression of absolute accuracy. Yet, if I hesitate at all in my praise of this very impressive study, it is because the reader never learns what it is to be human, to behave human, as an Ingalik. Precise, accurate details are preserved, but Ingalik patterns are ignored.

The problem of how to present ethnological data is one every ethnologist has to solve for himself. Osgood writes, "I . . . reserve the highest respect for those who review their basic assumptions most carefully." The basic assumptions of his analytical approach derive, I believe, directly from book-culture, and have little relevance in a world shaped by electronic media, and little appeal to a generation whose mentors are Joyce, Klee, and Pound, not Darwin, Marx and Freud—a generation interested in pattern, not atomized detail, and seeking relevant approaches to deal with existing data, not more data.

However, this criticism is irrelevant to the question of whether Osgood accomplished what he set out to do. In terms of the goal he chose, the Ingalik studies are brilliant successes and will be regarded as monumental studies by those who favor such a goal.

*Letters of Francis Parkman.* WILBUR R. JACOBS (Ed.) Norman: University of Oklahoma Press, 1960. Vol. I: lix, 204 pp., frontispiece, 13 illustrations, index. Vol. II: xl, 286 pp., frontispiece, 14 illustrations, index. \$12.50 (set).

Reviewed by WILLIAM N. FENTON, *New York State Museum*

Francis Parkman has always had a following among anthropologists. Some of us were attracted to the field by Parkman's ability to describe the conflict of cultures in North America and the sheer power of his narrative. Others met him in the undergraduate course on the American Indian through reading the *Oregon Trail* on the Plains, the *Jesuits in North America* for Algonquin, Huron, and Iroquois, and the *Conspiracy of Pontiac* for the central Algonquians. His vogue in American studies is not matched by an awareness of how he aided and abetted the pioneers in American ethnology. That history and ethnology were tied so closely a century ago is important to the history of the science and it affords perspective on the movement we now call ethnohistory.

The publication of the Parkman letters reveals the extent of his interest in American Indian studies, it discloses his relation to Morgan, Bandelier, and other pioneers, and affords the materials for understanding the sources of his interest and why and how he combined field work with documentary research to fulfill it.

It was thought that after Mason Wade, Parkman's biographer, found and published the *Journals* (1947) that discoveries were ended in Parkman's Beacon Hill study; but Wilbur Jacobs came upon a forgotten bundle of letters that inspired these volumes. Whatever one's interest in the 19th century, they make fascinating reading and are especially important for the student of personality and culture. What Parkman accomplished despite "the enemy," massive depression, and functional blindness, is amazing. Besides writing an illuminating introduction, which is a prospectus for a final biography, Jacobs has annotated each letter at the place it is printed, quoting the related correspondence, which has been traced to the originals. He has personally visited the sites of Parkman's field work, he has reread the sources available to the master, everything is beautifully cross-indexed, and I found only one wrong reference. Possibly both Parkman and Jacobs were seeking Samuel Kercheval's *A History of the Valley of Virginia* (Winchester, Va.: S. H. Davis, 1833, 486 pp.) (Vol. 1, pp. 32-33).

Before reading these letters the reviewer had not realized that Parkman originally considered "writing the history of the Indians, with the Iroquois as a central point" (Vol. 2, p. 213), "a people, more worthy of study than all the other Indian tribes united" (Vol. 1, p. 62), and to this end he had assembled a great mass of material (Vol. 1, p. 124). As an undergraduate he was inspired by reading Lafitau's *Moeurs des sauvages . . .* (1724), and just as Lafitau used the Iroquois to throw light on the peoples of antiquity, so Parkman thought that seeing the Sioux would help him to understand the Huron and Iroquois of the 17th century writers of the *Relations* (Vol. 1, p. xxxvi). His expedition to the High Plains had several results: the trip was a personal triumph, it gave him "a cartload of practical experience," he discovered that fieldwork among Indians was not his dish, but he came out of it a better historian, afterward able to appreciate accounts of savage life by others, especially by anthropologists for whom he developed genuine enthusiasm.

Parkman's interest in ethnology and archeology was deep and abiding. In an age when natural history was emerging and university students were being urged to study nature, not books, living Indian groups called the reader to quit the library and observe

them at first hand. He went to see Schoolcraft who advised him that personal knowledge of the Indian is essential to the historical writer who must not "rely on books alone" (Vol. 1, pp. 23, 52). But of the anthropologists he befriended and supported, his relations with L. H. Morgan and Adolph Bandelier, Morgan's protégé, gave him greatest satisfaction. Though in his review of the *League* (1851), Parkman had criticized Morgan for ascribing great wisdom to its unknown founders and for regarding as peculiarly Iroquois what is "common to many other tribes" (Carl Resek, in *Lewis Henry Morgan*, Univ. of Chicago Press, 1960, p. 44), Parkman would afterward acknowledge the value of Morgan's projected comparative studies, admitting that his own researches had a historical, not an ethnological object, to introduce his book on the Jesuit Missions, which Morgan deemed good ethnography at that (Vol. 1, p. 174; Vol. 2, p. 14). This correspondence elicited a comment on the place of pure research in American society of the day:

"It is refreshing to find in this country at this time one who has the animus and the courage to devote himself to a search after truth separated from questions of immediate social, political or economical bearing" (Vol. 2, p. 14).

This was after Parkman had arranged for Morgan to present his system before the American Academy of Arts and Sciences in Boston, whose membership received the paper in silence and afterward elected Morgan to membership (Resek, 1960, pp. 97, 106). These two scholars, historian and ethnologist, show the same divergence of interest and approach that characterizes the humanists and scientists in the ethno-history movement today. In reading Parkman's *Lasalle*, Morgan commented that French sources, which he found unsatisfying, when studied in their full range by Parkman, "can be made to yield satisfying results" (Vol. 2, p. 37); and Parkman praised Morgan for being first to use ethnology in historical criticism of the chronicles (Vol. 2, p. 36; Resek, pp. 132-133).

As for Bandelier, Parkman sent him money and lobbied for the support of his investigations. Bandelier acknowledged this help, but Parkman's letters of transmittal have not turned up. Any anthropologist who knows their whereabouts should communicate with Professor Jacobs.

*The Central Desert of Baja California: Demography and Ecology.* HOMER ASCHMANN. (Ibero-Americana, Number 42.) Berkeley and Los Angeles: University of California Press, 1959. x, 282 pp., bibliography, 10 figures, 9 maps, 16 plates. \$5.00.

Reviewed by WILLIAM C. MASSEY, *University of Florida*

We, in anthropology, are accustomed to much that is stimulating and much that is excellent in the contributions of the students of Carl O. Sauer and the Berkeley Geography Department. Aschmann's description and analysis of the human ecology, geography, and demography of the deserts of north-central Baja California is no exception. It is valuable to the specialist in western North America, and is of general anthropological interest both factually and theoretically.

The specific problem is that of explaining all of the factors relating to the cultural and demographic adaptations of humans to this desert area in the relative isolation of the central peninsula. This program is pursued with creative and thoughtful thoroughness. It entails: consideration of the environment; economic adjustments on all time levels; and the nature of the impact of European civilization in the Spanish Missions of the 17th and 18th centuries, with the subsequent devastation of the native Indian population. There emerges a concise study, the results and techniques of which are applicable in other areas.

The problem is stated in terms of human geography; the data are largely historical in derivation and anthropological in nature. But this is, after all, a "twilight zone" of definition. As anthropologists or geographers we should approach the materials in the same way with only a difference in emphasis.

The study involved both field work in the area and the necessary documentation for facts and interpretation. The presentation is organized according to nine aspects of the problem which range from an initial consideration of "The Physical Character of the Central Desert" to a short "Epilogue" concerning the 19th century post-Mission period adjustments of immigrant Mexican and foreign populations. Although the geographical approach is intended to be pervasive, there is a decided bias in favor of the aboriginal and Spanish scene. If there is any weakness it lies with the consideration of the modern peninsula.

There are several sections which deserve special comment. Chapter IV, "The Ecology of the Central Desert Indians" (pp. 58-132), contains the best description in print of the defunct Peninsula Yuman culture. Although this section emphasizes those cultural features which have a bearing on the ecological adjustment, it enthusiastically embraces a wealth of data on ceremonialism, social organization, and social interaction on the *rancheria*, or group, level. There occur occasional digressions from the problem at hand, as in the case of the extraneous description of capes and masks used by shamans, but they are additions to literature on the peninsula. This section is a fine presentation of the ecological orientation of various aspects of a desert-dwelling hunting and gathering people. It is all the more remarkable when we remember that the Indians have been extinct for over a hundred years. The data were laboriously culled from most of the 18th century Spanish documents on the area; this is not only the "lip-service" to such consultation which is common to many anthropological papers.

With the presentation of the cultural pattern of adjustment to this harsh environment, the demographic data on the native population in pre-Mission and Mission times are offered analytically in three sections. These include a consideration of gross population equilibrium, population density and distribution, and the fate of the Indians following the fatal inroads of disease during the Mission Period. These are problems which have especially fascinated Cook (1935, "Diseases of the Indians of Lower California in the Eighteenth Century." *California and Western Medicine*, 43:1-6, San Francisco, 1935; *The Extent and Significance of Disease Among the Indians of Baja California, 1697-1774*, Ibero-Americana: 12, Berkeley, 1937) and Kroeber (*Cultural and Natural Areas of Native North America*. UCPAAE: 38, pp. 177-179, Berkeley, 1939). Aschmann has taken advantage of the more sophisticated approaches of recent years, including some of the modern biological concepts, and his own analysis of the cultural adjustments of man in Baja California as a member of a biotic community. The population conclusions are more in line with those of Cook and Meigs III (*The Dominican Mission Frontier of Lower California*. UCPG: 7, Berkeley, 1935).

In sum, this volume combines productively the methods of geography, history, and anthropology. It is well done.

*Tropical Africa*. GEORGE H. T. KIMBLE. New York: The Twentieth Century Fund, 1960. 2 volumes, 603+506 pp., 47 illustrations, index, reference map, photographs, 66 tables. \$15.00.

Reviewed by PAUL BOHANNAN, *Northwestern University*

Seldom is a book so difficult to evaluate as is this one. Only when the whole is factored into many parts can a suitable judgment begin. First of all, it is important that a

book of this sort exist—and its importance has been generally heralded with praise in the popular press. Volume I, called *Land and Livelihood*, deals with geographers' problems: the physical geography, resources and their exploitation, population and its distribution, transportation, and marketing. Volume II, called *Society and Polity*, deals with social change, medicine, education, nationalism, new elites, and economic growth, under approximately twice that number of headings. The book does not purport to be primarily a source book, as Lord Hailey's *African Survey* does. It is to be read instead of merely being referred to.

It is also important that a book of this nature be based on accurate and adequate material. The list of contributors of working papers contains the names of many leading Africanists and would seem to assure quality. In spite of that fact, every specialist with whom I have discussed the book—and they are many—has major reservations about that part of it that deals with his speciality. Anthropologists, for example, will regret the new lease on life given to the old Seligmann classification of African "races," Hamites and all. They will squirm at the theoretical naïveté of the chapters on social and cultural change—the first two chapters of the second volume called "The Old Order" and "Social Change." Anthropologists who have worked in Africa will be little short of astounded at the misinformation about such topics as African agriculture—especially being told that it requires little labor, as in such passages as:

The Bemba . . . do about four month's agricultural work in the year. Some of the deep forest tribes manage to do even less. Bearing in mind the toll taken by disease of the African's vitality, this is fortunate; the chances are that many tribes, in their pristine condition, could not have coped with more intensive and sustained demands on their time and energies (Vol. I:133-36).

Geographers, political scientists, economists, and psychologists have pointed out passages that offend their specialties as much as this and others like it offend anthropologists.

Another difficulty can also be mentioned in this context. The Director of the Twentieth Century Fund, in his Foreword to the book, claims that it is well written. The point has been picked up by many reviewers in agreement. I must forbear. There is much "fine writing" here, but little straightforward exposition. Metaphors and images follow one another too rapidly, often at random, and sometimes inappropriately. Mr. Kimble writes well in that he writes picturesquely, not in that he selects clarity and economy as his goals. More seriously at fault than the style, however, is an attitude which is present throughout the two volumes. The above quotation shows it: "Some of the deep forest tribes *manage* to do even less." "The toll taken by disease of *the African's* [singular instead of plural] vitality. . . ." To one accustomed to discussing with Africans the literature about their continent, the book is almost impossible to evaluate for its information.

As one continues to read, the difficulty becomes apparent: it slowly dawns on the reader that the scope of this book is too big for a single man unless he be a lexicographer or compiler. This statement is not so much a reflection on Professor Kimble, who comes as respectably close to bringing it off as anyone could, as it is on the advanced state of the social and earth sciences of our time, for all that they are said to be backward.

And yet, it *is* Professor Kimble who provides what turns out to be the stricture in the book instead of what was undoubtedly meant to be the unifying single viewpoint to give it consistency.

In brief, this book must be on every Africanist's shelves, but he cannot recommend it to colleagues, students, or African friends without major reservations—like John Gunther's Africa, Kimble's Africa is too big for its breeches.



*The Eastern Lacustrine Bantu (Ganda and Soga)*. MARGARET CHAVE FALLERS. (Ethnographic Survey of Africa, Daryll Forde editor, East Central Africa, Part XI.) London: International African Institute, 1960. ix, 86 pp., appendices, bibliographies, charts, end map, index. 12s. 6d.

Reviewed by ROBERT F. GRAY, *University of Illinois*

The Kingdom of Buganda, lying north and west of Lake Victoria, is the largest and best known of the group of African states commonly called "Lacustrine" because of their location among the African Great Lakes. Busoga is smaller than Buganda and lies to the east across the Victoria Nile. The two districts together contain about three-eighths of the population of Uganda Protectorate. The author of this book has had first hand experience with both peoples and does an expert job of surveying the literature which, in the case of Buganda, is quite extensive with much of it written in Luganda. Since these groups have many culture elements in common, her method is to deal first with Buganda in discussing each topic and then to indicate the differences, if any, which are found in Busoga.

One difficulty with this comparative method is that Buganda and Busoga are not comparable political entities, Buganda having been a single unified state even in pre-European times, while Busoga was comprised of twenty or more independent political units, some having a population of only a few thousand. Although Busoga is now a single administrative district, it is difficult to see wherein its traditional unity lay. The northern Busoga states seem to have been closely related to the kingdom of Bunyoro culturally and linguistically, while the southern states were more akin to Buganda. This political difference does not much matter in a general cultural summary of the region, but it is the essence of the matter when we come to consider social and political aspects. The author is fully aware of this difficulty and proposes a hypothesis—which I found the most interesting part of the book—to explain the differences in scale and structure between Buganda and the Busoga states. The kingdom of Buganda has increased progressively in size through conquest and incorporation of neighboring groups, while in the case of the Busoga states the tendency has been towards division into smaller units. This is her interpretation concisely summarized:

Whereas Ganda princes were carefully excluded from political affairs and were distributed for totemic purposes among their mothers' clans, it was common practice for Soga rulers to give their sons control over villages, or even large territorial divisions. These grants tended to become hereditary and the result was princely enclaves with strong secessionist propensities (p. 67).

As this statement implies, the political structure, lineage system, settlement patterns, and principles of land tenure in each of these kingdoms are interrelated in a very complex manner. If the present book does little to clarify this complexity, the reason, I think, lies partly in the original sources which themselves are unclear and contradictory on these questions, and partly in the limitations imposed by the form of the Ethnographic Survey volumes. Thus the space that can be devoted to any subject is severely restricted, and the division of the material among the conventional topic headings tends to obscure rather than elucidate the social processes and structures which are the chief concern of contemporary anthropology with respect to African societies.

A valuable feature of the book is a critical examination of the widely held "Hamitic invasion" theory of the formation of these states. The author reviews the racial, linguistic, and cultural evidence for this theory and shows that when all the Lacustrine kingdoms are taken into consideration there is no simple correlation between these Hamitic factors and political power.

*Bamiléké de l'Ouest Cameroun.* CLAUDE TARDITS. Preface by HUBERT DESCHAMPS. (L'Homme d'Outre-Mer, New Series, Number 4.) Paris: Éditions Berger-Levrault, 1960. 136 pp., bibliography, 5 illustrations, map, 14 tables. 1.500 Fr.

Reviewed by PHYLLIS KABERRY, University College, London

M. Tardits was asked by ORSTROM in 1957 to submit a report on certain aspects of the "Bamiléké problem" and make recommendations. His book shows how much can be done in six months to survey a people numbering just under half a million, if a thorough knowledge of existing literature is brought to bear on administrative records and is refreshed by a brief field trip of three months. The treatment is admirably succinct but allows space for a scholarly and annotated list of sources (just under 400 documents), many of them unpublished but available in the Republic of Cameroun. The bibliography alone will prove invaluable for those planning fieldwork in the region.

The peoples who inhabit the Grassfields of the Cameroons are ethnically related, and many claim that their forebears migrated from the region of the Upper Mbam River and its tributaries in what is now the Republic of the Cameroun. In addition to a movement of peoples, there has also been a diffusion of material culture and social institutions. In consequence, the terms Mbum, Wute, Tikar, Bamum (really a subgroup of the Tikar), Bamiléké, and Chamba which are sometimes used to distinguish congeries of chiefdoms within the Grassfields, do not indicate exclusive ethnic units, or even ethnically homogeneous units. Nevertheless, a case can be made for retaining these terms to indicate culture areas and subdivisions within them. The Bamiléké chiefdoms, numbering over 90, lie to the west of the Nun River and extend to the present Southern Cameroons boundary. Apart from the work of Delarozière, Albert, Despois, and Hurault, little has been published on their kinship and political institutions; but it seems clear that, although they show affinities with the Bamum and other Tikar in the Southern Cameroons, they have more in common with one another—culturally and linguistically—than with their neighbors. As far as is known, such distinctive Bamiléké institutions as their style of architecture, *nkap* marriage, the proliferation of associations which form part of the machinery of government, and a special form of skull cult linked with a system of double unilineal descent do not occur among the Grassfields peoples of the Southern Cameroons. In the first part of his study, M. Tardits has analyzed the structural importance of these institutions in relation to chieftainship in its religious, political, and economic aspects in the traditional system.

Under the colonial régime, this structure was undermined by curtailing the powers of the chiefs in some directions and by making them the agents of the Administration for the collection of tax and, until 1946, the recruitment of labor. Christianity and education have created further disaffection; even more important, the growth of population and pressure on land have led to the emigration of more than 100,000 Bamiléké to urban centers such as Douala and to the Mungo valley. In Part II of the book, M. Tardits has brought together all the available demographic and economic material, much of it fragmentary and unsatisfactory. But it seems clear from his analysis that the total Bamiléké population may exceed 600,000 this year and that the problem of finding a means of livelihood in urban centers and on the land for this excess alone is acute. The whole situation is complicated by the violent disturbances which have dislocated life and communications in the Bamiléké region over the last few years; indeed, these disturbances are symptomatic among many other things of political disaffection in the chiefdoms and increasing pressure on economic resources. More representative political institutions will have to be introduced, and areas of resettlement

provided for emigrants and also for many in the chiefdoms who will require more land for coffee plantations if their present standard of living is to be maintained, let alone raised. M. Tardits' recommendations are practical and to the point, and his book should be of the greatest value to the administration. This book, along with his earlier volume dealing with the problems of the new élite in Dahomey and published under the title of *Porto-Novo* in 1958, also establishes his reputation in the field of applied anthropology.

*Social Bandits and Primitive Rebels: Studies in Archaic Forms of Social Movement in the 19th and 20th Centuries.* E. J. HOBBSAWM. Glencoe: The Free Press, 1959. vii, 208 pp., appendix, 3 indices. \$5.00.

*Reviewed by* JULIAN PITT-RIVERS, *University of Chicago*

This book is concerned with popular agitations and their adaptation to modern economic conditions, that is to say, the ways in which "primitive" or "pre-political" forms of rebellion give way to "modern" political parties and trade unions. The social bandits of the title are those Robin Hoods of whom examples are found at one epoch or another in most European countries, and whose exploits against the rich and sympathy with the oppressed make them in the popular imagination heroic symbols of rebellion. Mr. Hobsbawm deals primarily with Italy and Spain and gives us, as examples of social protest belonging to a primitive era, not only bandits but the Mafia and three millenarian movements: the Tuscan Lazzarettists, the Andalusian Anarchists, and the Sicilian Fasci. There follows a chapter on the preindustrial city mob, the best essay in the book, another on the relation between religious sects and the labor movement in Britain, and a final chapter on "ritual in social movements," which shows how it has died out in the transition from the primitive stage to the modern epoch.

The author has, most laudably, endeavored to get the "feel" of the people he writes about by going to visit the places. The common "feel" of the phenomena described is an adequate reason for grouping these essays into a single book, even though one may wonder to what extent it is simply the feel of "primitiveness." "It is, as it were, an historical accident that the bandits, the *Mafiosi*, the Lazzarettists, the Sicilian peasant Socialists or Andalusian peasant anarchists found themselves living in the 19th and 20th centuries instead of the 14th." Such analogies can enlighten but they can also confuse, as the author points out in referring to Professor Norman Cohn's comparisons between the millenarians of the 14th century and more recent revolutionaries. Yet, insofar as it aims "to attract attention to certain topics which have been very little studied and are still largely unknown," this book must be welcomed as an unqualified success. The material is fascinating, the style lively, and the analogies are frequently perceptive.

However, the book also aims at interpretation by the use of a theory of evolution whose stages are: tribal, where kinship ties are strong and rebellious energies go into the vendetta rather than into social protest; then, a second stage, to which the primitive rebels and social bandits belong which is no longer tribal but is still "pre-industrial" and "pre-political" and in which the impact of modern industrial conditions is felt. It is this which predisposes such people to rebellion whether "reformist," as in the instance of the city mob and the Robin Hoods who accept the traditional order and rebel against those whom they regard as its corruptors, or "revolutionary," as in the millenarian movements which await the coming of a new social order. This stage is the "threshold of political consciousness" yet it lacks the possibility of any effective political action. It is followed by the age of modern political organization which is assumed to be Marxist. Nationalistic movements are not dealt with and, when the populace of Naples voted

Monarchist rather than Communist in 1956, this is interpreted as "lack of interest in modern politics."

Such evolutionistic theories, it has been pointed out, lend themselves to circularity, and it must be admitted that Mr. Hobsbawm indulges in a certain amount of circularity of the type: Why did they not rebel?—Because they had not attained political consciousness—How do we know that?—Otherwise they would have rebelled. This scheme also obliges us to accept the existence in the same community simultaneously of phenomena classed as belonging to different stages: the preindustrial Mafia in the American city (where it becomes something called "pure crime"); the Andalusian bandits after the Civil War, no longer prepolitical since endowed with political ideology and support but otherwise conforming in their behavior to their traditional fore-runners. But the reader should not be put off by this apparent arbitrariness. The hard facts remain that there is a historical progression from one kind of consciousness to another (this is well illustrated in the study of the labor sects), and that this is connected with changes in the economic structure of the country. The question is whether the categories used by the author are best able to explain the process. It appears to me that the scale of political consciousness is more important than the content and that, viewed in terms of the moral horizon of the community, some of the apparent anomalies disappear. Mafia requires not preindustrial conditions nor prepolitical thinking but a closed community, and the underworld is always this. It also appears to me difficult to generalize regarding this process without examining the power structure of the local community. It is to be hoped therefore that these studies will be followed up by more detailed work.

*Società e Uomo in Sardegna: Ricerca di Sociologia Positiva.* MICHELE MAROTTA. (Annali Economico Sociali della Sardegna, Serie 1, Volume 1.) Cagliari: Regione Autonoma della Sardegna, Assessorato all'Industria, Commercio e Rinascita, 1958. xvii, 413 pp., appendices, indices, maps, tables. n.p.

Reviewed by STEPHEN C. CAPPANNARI, *Vanderbilt University*

This monograph is a study of man and society in Sardinia within a wide variety of categories of data and related theoretical discussions. Marotta, who is professor of sociology in the University of Cagliari, includes, e.g., data concerning selective factors among immigrants to Sardinia; the relationships of height, intelligence, and the altitude of the place of birth of Sardinians; between illiteracy rates and occupation, etc. Categories such as these are, in turn, related to a conception of the individual and society in equilibrium. Additional evidence of Professor Marotta's wide range of interest is furnished by the many hundreds of references to diverse American as well as European scholars.

The basic theoretical orientation of this book stems, however, from the statistical-sociological school which was originated by Professor Corrado Gini at the University of Rome. It is written in a lively style and should interest those concerned with methodology among Italian social scientists, as well as those with specific interest in Sardinia.

*The Hoe and the Book: An Italian Experiment in Community Development.* FREDRICK G. FRIEDMANN. Ithaca, New York: Cornell University Press, 1960. xxi, 158 pp., index. \$3.00.

Reviewed by SYDEL F. SILVERMAN, *Columbia University*

The Unione Nazionale per la Lotta contro l'Analfabetismo, a voluntary association

created in 1947 by a group of Italian teachers and social workers, was conceived as a struggle against much more than illiteracy. In the wake of the postwar concern with the neglected Mezzogiorno, it dedicated itself to bringing a new atmosphere of cooperative solidarity, self-determination, and individual responsibility into the rural South. In his elegant, moving little book, Professor Friedmann critically examines the developments and problems, achievements and challenges of the first ten years of its activity. We are led through UNLA's contacts with, and attempts to, counter the whole by now familiar panorama of ills of the region: the traditions of continued dominance by distant powers, the entrenched and insidious two-class system, the bureaucracy, the clientele system—perpetuated by both clients and patrons, the exploitation and ineptness of the national government, the utilization of law as an instrument of the powerful, the lethargy and mutual suspicion among the people, the isolation of families, the degrading position of women and the frustrating relationships between the sexes, the antipathy towards manual labor, the interpretation of education as a possession of the privileged and as an avoidance of reality. The immensity of the task is underscored by a summary, based on the author's earlier work, of the ethos of the peasant world of *la miseria*, with its curious balance of broad humanistic solidarity and narrow destructive individualism.

UNLA's approach was a unique one: it saw in *la miseria* itself a common bond among men which could form the basis for continuous, cooperative social action. In the process of operating the literacy courses, general discussion groups, technological and vocational workshops, recreational activities, even the welfare assistance and the trips northward to more democratic nations, an underlying aim was to reaffirm the positive values that were believed to have long existed in the culture, though perhaps in dormant form. In this sense, in spite of the fact that UNLA apparently was guided more by faith than by explicit theory, its work may be instructive to applied anthropology, as an example of controlled culture change on the basis of intrinsic rather than imposed values.

To what extent did the experiment succeed? The progress made in reducing illiteracy was not outstanding. However, definite advances were noted in more productive use of leisure time, and some stirring examples could be cited with regard to the development of attitudes of cooperation and self-help. In its own operation, the *Unione* was surely guilty of some of the same paternalism, undemocratic organization, and isolation of the social classes which it sought to defeat. The author justly concludes that these limitations are of small import in comparison with the enthusiasm it won among its associates and students, though he sees in this very enthusiasm a possible pitfall in UNLA's future adaptation to changed requirements in a society no longer isolated from and neglected by the mainstream of a technologically advanced national life.

Professor Friedmann's treatment is eminently fair. In a laudable preface, he takes pains to indicate aspects of his background and beliefs which might have influenced his interpretation. As the author himself suspects, he does not entirely avoid romanticizing the "sacred drama" quality of UNLA's efforts and the peasant in his "acceptance of a fated order." Nevertheless, we are left with a balanced picture of an experiment that fumbled as well as triumphed, of peasants with a reasonable share of unsavory characteristics, and of a rural world which is always seen in relation to the more comprehensive society and which includes a great deal more than "peasants."

Much of the literature dealing with South Italy seems to point to the definition of a "syndrome" of social ills. For all the dismay this provokes among social scientists interested in this area of the world, they have yet to provide an adequate analysis of it. This incisive contribution of a philosopher to the task will be welcomed.

*Oasis and Casbah: Algerian Culture and Personality in Change.* HORACE M. MINER and GEORGE DE VOS. (Anthropological Papers, Museum of Anthropology, University of Michigan, No. 15.) Ann Arbor: University of Michigan, 1960. v, 236 pp., appendices, bibliography, 10 figures, index, 15 tables. \$2.50.

*Reviewed by* JOHN GULICK, *University of North Carolina*

The publication of this valuable monograph brings us close to getting a good triangulation on some aspects of personality in Arab culture, since it joins the ranks of Hamed Ammar's study of personality formation in Upper Egypt and of Herbert Williams' and Terry Prothro's unpublished studies in Lebanon. These four studies give us a fair geographical sampling of the culture and have produced results which are similar in certain important ways. This is encouraging since they were conceived and executed independently.

Horace Miner spent nine months in Algeria in 1950. In addition to making ethnographic observations, he obtained Rorschach protocols from 20 men who had lived virtually all their lives in a small town on the edge of the Sahara, 28 men who had been raised in this town but had subsequently been living in Algiers and/or France for at least a quarter of their lives, and 16 men who had returned to the oasis after having lived for some time in the city. The first two groups ("oasis" and "urban") are those on which the analysis of change is primarily based. In general, the personality differences between them, as revealed by the "blind" analysis of the Rorschachs by De Vos, are not striking except in the urban peoples' greater intra-psyche tension and hostility which are attributed to the aggravated pressures of the French in the city. Since the cultural conditions of the typical life cycles through adolescence in both samples had been very similar, the minimization of differences in the Rorschach findings is hardly surprising. Consequently, the authors' emphasis on personality in change is as disconcerting as its results are anticlimactic. If Miner had selected an urban sample which had no oasis contacts and displayed more clear-cut nonoasis cultural features, he would have given himself a better chance to demonstrate oasis-urban differences which are more fundamental at the personality level.

But this is an important work regardless of the element of misplaced emphasis in it. Miner and De Vos have made available a profile of 64 Algerian men who represent that segment of the population which is primarily rural in background but is participating to various difficult-to-differentiate degrees in the urban milieu. This is an important element not only in the Algerian population but in that of the Arab world generally, not to mention the modernizing peasants throughout the world.

The profile is of a personality which is basically insecure. Smothered by maternal physical affection in infancy, it is abruptly thrust into a male social environment in which it must subject itself to the aggressive discipline of the father and others, and it gradually learns to make its own way either by becoming essentially submissive or by becoming itself overweeningly dominating. The behavioral models of coping which are set by both parents are those of emotional outbursts rather than logical perseverance. The adult is afraid of showing submissiveness, which he associates with femininity, but, particularly if he actually is prone to submissiveness, he compensates with bravado and by taking satisfaction from his submission-by-seclusion of actual females. Taking the passive role in homosexual relationships, coupled with intense shame at doing so, is the ultimate in this configuration. In many of the Rorschach scores these people are in the American neurotic and psychotic ranges. In terms of psychosexual development, the personality is more anal than oral.

Miner does not carry his conclusions any further than a descriptive summary. And yet there are several important interpretative problems which the study raises. One of these is the same that arises in every case of a cultural study in which psychoanalytic concepts and projective tests are used: the culture and/or the typical personality always appear to be emotionally pathogenic. Is this not inevitable when concepts and methods are used which were designed expressly for diagnosing deviations from the elusive psychic "normalcy" of a particular culture? Therefore, is not the use of these techniques and concepts in cultural analysis an insidious form of differential evaluation of cultures? In view of the way anthropology is now practiced, the answer to both questions is yes. Is anthropology at present able to cope conceptually with the implications about the validity of "cultural relativism" (i.e., equality before the anthropological law) which can be drawn from this answer? I do not think it is except by dint of frankly rejecting the idea of "cultural relativism," and this would be an oversimplification of reality as we now understand it. This issue may not have been compelling when it only concerned the reported paranoia or anal or oral fixations of small, isolated, primitive cultures. But now it also concerns societies which are active agents in the political and economic affairs of a large proportion of mankind. It involves people who have national friends and enemies, people who care very much what is said about them and who says it.

In the present instance, the matter is even more compelling, for it is quite possible that a good deal of Miner's depiction of Algerians is generalizable to the Arabs as a whole. If it is, the depiction will certainly contribute to some widely held pejorative stereotypes of the Arabs. In other words, while it contributes to one kind of understanding, it also aggravates another kind of misunderstanding. Such consequences of limited knowledge raise more than merely academic problems. These, anthropologists can, if they will, try to counteract. Marvin Opler puts his finger on the central issue in his introduction to *Culture and Mental Health*, a book which includes a précis by Miner and De Vos of the monograph under review. Opler says, "Since such evaluations or ratings . . . rarely consider people's assets along with their liabilities, much is lost in many studies concerning human resistances to illness." While it is true that the Arabs have not coped very effectively so far with several virulent forms of European aggressiveness (and Miner's study helps us to understand some of the reasons why), there is hardly a culture without assets, as its long persistence at the meeting point of three continents plainly attests.

Let us hope that anthropology will develop techniques for the elucidation of a culture's assets which are as ingenious as those which it has been using to elucidate a culture's weaknesses.

*Saudi Arabia: Its People, Its Society, Its Culture.* GEORGE A. LIPSKY and others. (Survey of World Cultures Series.) New Haven: HRAF Press, 1959. 367 pp., 4 maps, 11 tables. \$7.00.

Reviewed by CARLETON S. COON, *University Museum, University of Pennsylvania*

Writing a handbook about Saudi Arabia is not an easy task. Whoever undertakes it has to avoid offending many persons and organizations. The Saudis themselves have shown no inclination to like being talked about. Their ruling class in particular regards such an effort as an invasion of privacy. The oil companies working there are also understandably loath to have material released through their offices which will embarrass their hosts, and certain Jewish groups do not like the exclusion of their co-religionaries from the kingdom.

The authors, one of whom is an Arab, have done an excellent job of walking this three-cornered tightrope without falling into the net of embarrassment or offense, or so it seems to this reviewer. They have given a clear, factual exposition of the history, geography, demography, religion, social structure, economics, financial structure, industry, trade, and other aspects of the life of Saudi Arabia, which should serve as a guide to any interested person, and all in nontechnical language. But they have done it at a high price—that of boredom. The book is written in such a cautious, objective and completely dull style that it is hard, at least for someone who has been there, to read. The language is what might have been produced by a robot or a computing machine. Therein, it seems, lies its innocuous quality.

*USSR: Its People, Its Society, Its Culture.* THOMAS FITZSIMMONS, PETER MALOF, JOHN C. FISKE and others. (Survey of World Cultures, No. 7.) New Haven, Connecticut: Human Relations Area Files Press, 1960. 590 pp., appendix, bibliography, index, 12 plates, 2 tables. \$8.50.

*Reviewed by* STEPHEN P. DUNN, *New York*

The authors of this general survey (the seventh in the Human Relations Area Files Series) undertake to produce "a collation and synthesis of the best and most authoritative material published and unpublished on the societies selected"—in this case, the Soviet Union. Their work is based almost entirely on secondary sources; examination of their mountainous bibliography reveals only a handful of Soviet items, most of these standard reference works.

The authors state that for financial and other reasons they are unable to document in detail the points made. Unavoidable as this circumstance may have been, it seriously limits the book's value to the specialist. For the layman, on the other hand, the book is likely to be misleading, since, like most American publications dealing with the Soviet Union, it is shot through with the effects of prejudice and of our own particular brand of ideology. For instance, the authors state in their introductory chapter, "Perspectives," that: "Many of the tensions in Soviet society arise from the fact that the regime has forced upon its citizens ways that are foreign not only to their tradition but to their character" (p. 1). This statement is at the least a drastic oversimplification; the reviewer believes that in this unqualified form it is utterly false. Indeed, the authors appear to recognize this implicitly when, throughout the book, they point out the ways in which the regime has used traditional Russian habits and values, and has also been forced to modify its policy in order to accommodate these values. Toward the end of the book they state specifically: "The concentration of absolute power in the state is part of [the Russian people's] heritage and is associated with the greatness and power of the nation itself" (p. 432). These and similar qualifications apply to non-Russian peoples in the Soviet Union as well as to Russians—a point which the authors fail to make. Nor is the reader reassured when, immediately following the statement quoted from page 1, they proceed to give a psychological interpretation of Russian character, apparently based on the work of Mead and Gorer—taking no account of the serious objections which have been raised to this work.

The first major subdivision of the book, entitled "The Social Setting," contains most of the material of anthropological interest, and most of that which this reviewer, as a student of Russian anthropology, is particularly competent to judge. The treatment of the social structure and culture of the non-Russian peoples, and of the handling of them by the regime, shows extreme oversimplification. The authors state, for instance,



that: "Most of the nationalities of the North are of minor importance to the regime, and little effort has been expended to change their culture, except when it seemed economically advantageous to the regime to do so" (p. 99). This statement—and in fact the whole book—ignores a vast Soviet ethnographic literature describing, often quite circumstantially and objectively, the efforts made from 1920 on to modify the culture of the non-Russian peoples along ideologically predetermined lines; these efforts have met with a good deal of success, considering the natural and cultural obstacles involved. The literature in question is largely unknown in the West; the bibliography of the book under review contains nothing which, as far as the reviewer knows, would give detailed reference to it (with the possible exception of certain reports not generally available even to the professional public). It seems a pity, however, to perpetuate this situation when an opportunity exists to correct it.

The central sections of the book, consisting largely of political and economic exposition, are not open to such serious objections from the anthropologist's point of view. However, they are in no way superior in their treatment of the subject to standard sources like Fainsod's *How Russia Is Ruled*, Hazard's *Law and Social Change in the Soviet Union*—or indeed any one of a number of books cited in the bibliography.

The authors include an appendix on "Peoples and Languages of the USSR," compiled in gazeteer style and containing such information as could be found in any good encyclopedia. All in all, the value of this book, even as a reference compendium, is negligible, and there would seem to be no good reason for having written it or put it on the market, particularly at so exorbitant a price.

#### ARCHEOLOGY

*Archeological Investigations in British Guiana.* CLIFFORD EVANS and BETTY J. MEGGERS. (Bureau of American Ethnology, Bulletin 177.) Washington: Smithsonian Institution, 1960. xii, 418 pp., 68 plates, 127 figures, 53 tables. n.p.

*Reviewed by* IRVING ROUSE, *Yale University*

Evans and Meggers worked in British Guiana during 1952–53 as a sequel to their 1948–49 excavations at the mouth of the Amazon River (B.A.E. Bulletin 167, 1947). They were searching for evidences of "migration or diffusion" up and down the coast between the mouths of the Orinoco and Amazon rivers and of "prehistoric communication between the coast and the interior" via the Guiana Highlands (pp. 1–2).

The greater part of their work on the coast was done in the Northwest District, a swampy area adjacent to the delta of the Orinoco River. Here, in a 1944 reconnaissance, Cornelius Osgood had encountered remains of both preceramic and ceramic occupations and had been able to show that the latter was probably an offshoot of the Barrancoid ceramic tradition on the lower Orinoco River (Yale University Publications in Anthropology 36:57–60, 1946). Evans and Meggers now succeed in defining Osgood's preceramic occupation more precisely as an Alaka phase and his ceramic occupation as a Mabaruma phase. They also distinguish a second ceramic phase, Koriabo.

The preceramic Alaka phase is characterized by crude, percussion-made choppers, picks, and scrapers, with ground stone tools such as celts and milling stones added in the later deposits. The authors compare this phase with the three-fold Manicuaroid series of complexes in Venezuela as follows (p. 146):

The closest sites geographically are on the east coast of Venezuela. Here the best known complex [sic] is the Manicuaroid [sic] series (Cruxent and Rouse, 19[58–]59:240–241), the diagnostic artifacts of which are said to be shell gouges and bone projectile points. Neither of these artifact types was encountered in the Alaka Phase excavations, but this may be a function of the relatively

small amount of digging here as compared to that of the Maniquaroid sites, where rocks and chips that might be crude artifacts of the type associated with the Alaka phase were apparently not brought into the laboratory for more detailed examination.

This statement contains two errors. The nearest preceramic sites are not on the east coast of Venezuela but in Trinidad, just off the mouth of the Orinoco River, where John M. Goggin and the reviewer have defined a distinct, Ortoire complex (Yale University Publications in Anthropology 50:94-96, 1953). In the case of both the Maniquaroid series and the Ortoire complex, all possibly worked stone artifacts, as well as representative samples of the unworked stones, were brought into the laboratory and are available for study. Indeed, the crudely worked stone artifacts are illustrated in the publication cited by Evans and Meggers (Cruxent and Rouse, *An Archeological Chronology of Venezuela*, 1958-59: Pls. 5:2,3,9,10; 6:8; 41:26; 42:2-5,7). Neither in Venezuela nor in Trinidad do these crude, percussion chipped artifacts resemble those of the Alaka phase, and hence the reviewer is unable to accept Evans and Meggers' suggestion that the Alaka phase may represent "an intrusion from the Venezuelan coast" (p. 146).

Evans and Meggers cite two radiocarbon dates for the Maniquaroid series:  $2,760 \pm 130$  and  $2,450 \pm 90$  B.P. Unfortunately, both are considerably more recent than the three radiocarbon dates actually obtained for the series; one of them refers instead to the Ortoire complex (for which two dates were obtained) and the other to a later preceramic complex known as Pedro García (Cruxent and Rouse, 1958-59: Tables 2-3).

Evans and Meggers' own guess dates for the Alaka phase are even later: A.D. 1-500. In terminating the phase at the latter date, they implicitly reject Osgood's tentative correlation of the preceramic culture with the modern Warrau Indians. The reviewer would not be surprised, however, if more recent nonceramic material turns up in British Guiana, since the Warrau themselves lacked pottery in early historic time.

The authors approach the ceramic phases by means of James A. Ford's methods of ceramic typology and seriation. As in their Amazon report, they handle this approach very well indeed and thoroughly document it with descriptions, tables, charts, and other illustrations, which comprise the bulk of the volume. For the Mabaruma phase, they seriate 32 site units—whole cuts or levels in cuts—producing a sequence which they estimate to have lasted from A.D. 500 to 1600 (Figs. 48, 126).

The authors use changes in the modes of vessel shape and design as "the basis for subdividing the Mabaruma Phase sequence into early, middle, and late segments" (p. 147). From the reviewer's point of view, these segments might better be called phases (or, in more purely ceramic terms, styles) and the entire sequence a tradition (or series). Reformulated in this way, Mabaruma becomes a classic example of the development of a tradition. Its earliest phase, as the authors point out, is strongly Barrancoid, reflecting its probable origin in the latter tradition on the lower Orinoco River or in Trinidad. The middle and late phases apparently become more and more divergent, with the result that Mabaruma comes into its own as a separate tradition. The authors attribute the divergence to influences from other regions, of which they cite several examples. The reviewer is inclined to think that mere physical separation of the Mabaruma people from their Barrancoid ancestors may also have been a factor, since it should have fostered divergent development.

The authors' analysis of the pottery into modes, while not intensive, is sufficient for their purposes. More detailed comparisons of the ceramic sequences in British Guiana, on the lower Orinoco River, and in Trinidad, all of which appear to be interrelated, had best await completion of the reviewer's long-overdue Trinidad report.

Koriabo, the other ceramic phase in the Northwest District, belongs to a different

tradition. The authors relate it to unpublished material dug by Peter Goethals in Surinam and conclude that it moved up the coast from that direction, contrary to the Mabaruma tradition (Fig. 126).

The only other part of the coast in which the authors worked was on the Abary savanna, towards the border with Surinam. Here they encountered a single, Abary phase, which they consider to be contemporaneous with Koriabo and late Mabaruma (Fig. 126). For its origin, they look up the coast to the island of Trinidad, where John M. Goggin excavated somewhat similar cariapé-tempered pottery at the site of Mayo (Fig. 127). But the authors note a flaw in their theory. Mayo is the site of a mission, known to have existed from A.D. 1687 to 1789, whereas the Abary phase is estimated to have entered British Guiana "as early as A.D. 1000-1200" (p. 188). The authors suggest that the identification of the mission "is incorrect or that the sherds represent an earlier occupation not associated with the historic community." However, Goggin (personal communication) found not only European trade objects at Mayo but also Indian pottery made in imitation of Spanish artifacts. Moreover, Mayo is the only one of 30-odd ceramic sites on Trinidad which has yielded an appreciable amount of cariapé-tempered sherds and hence its pottery can best be regarded as intrusive. The reviewer sees no reason to reject Linné's thesis (in his *Techniques of South American Ceramics*, 1925:44) that cariapé tempering "was invented in the culture area around the lower Amazon."

In their previous, Amazon report (B.A.E. Bulletin 167, Fig. 206: 1957), Evans and Meggers had postulated a migration from the Lesser Antilles down the coast of the Guianas to the mouth of the Amazon in order to account for certain similarities between various cultures in the Greater Antilles and the Aruã phase at the mouth of the Amazon. Since they do not mention this theory in the present volume, it is to be presumed that they have abandoned it. Certainly, their finds along the coast of British Guiana do not support it. In the reviewer's opinion, such theories of long-scale migration are premature and will remain so unless and until they can be supported by excavations spotted regularly along the coasts of all the Guianas.

The historic distribution of tribes along the coast of British Guiana is poorly known, and the authors wisely refrain from attempting to identify any of their phases except Abary, which they consider to be Arawak. They do not attempt to reconcile the Abary identification with Osgood's contrary identification of the Arawak tribe with the Mabaruma tradition, which better fits the situation on the lower Orinoco River and in the Antilles, where the related Barrancoïd pottery has likewise been correlated with the Arawak.

Evans and Meggers investigated two districts in the highlands, the Rupununi savanna and the rain forests along the upper Essequibo River, both of which are close to the Brazilian border. On the savanna, they located a single chipping station, which may date from Paleo-Indian time. In this connection, they summarize the scattered finds of presumed Paleo-Indian projectile points in British Guiana and make comparisons with finds in other parts of South America without, however, mentioning the El Jobo complex of Venezuela, which contains some similar projectile points (Cruxent and Rouse 1958-59: Pl. 20:1-2). They conclude that "in view of the wandering life associated with a hunting economy and the unexcelled opportunities for concealment of artifact remains provided by the forested terrain, it is doubtful that our knowledge of these hunting cultures in British Guiana will ever be much more complete than it is at present" (p. 124). Recently, however, Cruxent (personal communication) has found abundant remains of an El Jobo-like complex in the Venezuelan Guianas, and it would seem likely

that similar materials will eventually turn up across the border in British Guiana, especially in nonforested areas such as the Abary and Rupununi savannas.

The authors encountered two ceramic phases in the highlands, Rupununi on the savanna of that name and Taruma in the Essequibo rain forest. They are successful in identifying these with tribes which moved into British Guiana from Brazil during historic time. Not only are they able to use ethnographic and ethnohistorical data to supplement their archeological descriptions but also they date a number of the sites from the sources. This in turn enables them to continue the experiment they began at the mouth of the Amazon of estimating the length of occupation of a site from the number of potsherds which it contains, for they can check the estimates with the sources. Finally, by digging two sites recently abandoned by the Wai Wai Indians they have obtained interesting information on the disintegration of material culture in a tropical environment.

Their highland investigations leave a gap between the Paleo-Indian horizon, possibly as old as 10,000 B.C., and the appearance of the two ceramic phases about A.D. 1600 (Fig. 126). They are not concerned by this gap, however, for they believe that

it substantiates the hypothesis that the Guianas functioned as a refuge area rather than as a fountainhead of Tropical Forest cultural development as was once postulated . . . The antiquity of settlement by Tropical Forest [pottery making] groups is greatest at both margins of the area—the mouth of the Orinoco and the mouth of the Amazon—implying primary migrations and/or diffusion down these two major rivers from the west. Spread along the coast of the Guianas appears to have been incredibly slow, and towards the interior practically nil. In fact, it is an open question whether the Rupununi savanna or the upper Essequibo would have yet been invaded by Tropical Forest culture had the aboriginal balance not been upset by the arrival of Europeans (p. 340).

This conception of Guianan culture history conflicts with that of Cruxent and the reviewer (1958–59). We see the Orinoco Basin—although not necessarily the Amazon—as a center of “Tropical Forest” cultural development, parallel to the “Formative” development in western South America. Following Sauer (*Agricultural Origins and Dispersals*, 1952:40 ff.), we assume that this Tropical Forest development was based upon the domestication of bitter manioc, whereas the Formative development was touched off by the domestication of maize.

Our series of 30 radiocarbon dates, supplemented by other lines of evidence, indicate that Tropical Forest culture was already present on the lower Orinoco by 1000 B.C. and that it had spread to the east coast of Venezuela by the time of Christ, moving into the Antilles shortly thereafter. The reviewer would be surprised if it had not also reached British Guiana by the time of Christ rather than at A.D. 500, as postulated by Evans and Meggers, and if it did not spread further and more rapidly through the Guianas than they assume.

According to Evans and Meggers, “it can confidently be predicted that no pottery-making culture [will be found in the Guianas] earlier than the Mabaruma phase” (p. 340). The reviewer is not so confident. On the Orinoco, we found a succession of two Tropical Forest traditions, Saladoid and Barrancoid. It was the Saladoid tradition which spread to the coast of Venezuela and out into the Antilles about the time of Christ. May it not also have spread into British Guiana, earlier than the Barrancoid-derived Mabaruma tradition? The reviewer doubts that a single series of excavations such as Evans and Meggers, in a country so large as British Guiana, is sufficient to eliminate this possibility.

Evans and Meggers do not discuss the discrepancies between their conclusions and ours except to criticize us for saying that maize was “absent” from our postulated

Orinocan cultural center (p. 341). This is a misquotation; we actually stated that the agricultural complex of the Orinocan center "emphasized" bitter manioc, whereas maize dominated in western Venezuela and Colombia (Cruxent and Rouse 1958-59:264). We could hardly have said more, since we were projecting the historic situation back into prehistory, and the cultivation of maize as a secondary crop is well attested on the Orinoco and in the Antilles during the historic period.

It is our contention that, since bitter manioc is so well adapted to tropical forest conditions in eastern South America, its presence there as a staple food permitted earlier and higher development of culture than Evans and Meggers are willing to admit. The reviewer doubts, for example, that Tropical Forest culture developed any later or any less intensely than Woodland culture in the eastern United States.

In postulating diffusion of Tropical Forest culture down the Orinoco Valley, rather than local development in the valley, Evans and Meggers follow Julian Steward's Circum-Caribbean theory (Southwestern Journal of Anthropology 3:85-107, 1947), in which it was assumed that Formative culture arose in the Andean highlands, moved into the eastern lowlands, and there gave rise to Tropical Forest culture. But the first part of this assumption has been outmoded by recent excavations in lowland Guatemala and Ecuador, which suggest that the basic elements of Formative culture originally diffused along the Pacific coast and that the Andean highlands were at the time a peripheral region (M. D. Coe, American Anthropologist 62:390, 1960). In other words, the highlands may well have served as a barrier between an independent Tropical Forest development in the east and the diffusion of Formative culture in the west. Indeed, if Gordon R. Willey is correct (Science 131:74-6, 1960, there were two successive stages of cultural development in the tropical lowlands, both to the east and to the west of the Andes, before the appearance of comparable culture in the highlands: (1) incipient agriculture and/or pottery, and (2) intensive agriculture of the Tropical Forest type in the east and of the Formative type in the west.

This is not to say that the Andean highlands lagged in cultural development at a later date. But could not the Tropical Forest and Formative cultures eventually have come together in the highlands, enriched each other there, and formed a secondary center of development in the highlands? Speculative though this is, it seems to the reviewer a more reasonable hypothesis, given the chronological evidence summarized by Willey (1960), than to assume that the highlands were the source of all Neo-Indian cultural developments in South America.

These criticisms of theory are not intended to disparage the value of "Archeological Investigations in British Guiana" as a report of field work. As such, it is solid, sound, and of high quality. It only needs to be followed up by additional work of the same kind, directed towards specific regions such as the Northwest District and designed to settle the questions of interpretation raised above.

*Ancient Mexico.* FREDERICK A. PETERSON. Maps and drawings by JOSÉ LUIS FRANCO. New York: G. P. Putnam's Sons, 1959. 313 pp., bibliography, 93 figs., 24 plates, index, 6 maps. \$7.95.

Reviewed by MICHAEL D. COE, Yale University

To any teacher of Mesoamerican or even New World archeology, the lack of modern texts in English on Mesoamerica as a whole is striking. Valliant's *Aztecs of Mexico* is now hopelessly out-of-date, with its faulty identification of the Toltecs with Teotihuacan and the distorted Morgan-Bandelier version of Aztec society which it presents. While marred by minor typographical errors, the great posthumous work of Covar-

rubias, *Indian Art of Mexico and Central America*, is the only single work which does justice to both Mexico and the Maya area, taking into account modern ideas (many of which were those of Covarrubias himself) on Mesoamerican archeology. Its forbidding price, however, is considerably higher than most students can or will pay for a text.

It was to be hoped that *Ancient Mexico* would fill the need for an up-to-date text, but unfortunately it cannot be recommended to any but the general public. On the debit side we must list: (1) the absence of references or even an indication of which books in his general bibliography may be consulted with profit by the interested reader; (2) the absence of some recent developments in Mesoamerican archeology, including the author's own work at Chiapa de Corzo, despite a 1959 dateline; (3) the random distribution of the plates, which are scattered throughout the book without regard to period or culture. Most serious of all is the organization of the book itself. The first five chapters comprise a popularly written but adequate account of Mesoamerican history (in which the Maya civilization is given such short shrift—a page and a half only—that it might have been best to eliminate it altogether). The final 11 chapters are journalistic treatments of general subjects such as "Warfare and Warriors" (Chapter VIII) or "Art and Artifacts," which are so superficial and sketchy, except where post-Conquest information is utilized, as to be quite useless. Neither in the chapter entitled "Pottery and Potters" nor anywhere else in this book do we get any account of the evolution of pottery forms and decoration in Mesoamerica, where at one time or another in its development almost every conceivable shape was made and used.

To continue in the same vein, one might take issue with many statements made by Peterson throughout this volume. On page 20, he says, "There were at least two large groups of migrants to America. One group was long-headed, and another was broad-headed"—an outmoded, pregenetic idea for which there is no justification. The claim is made on pp. 58-59 that: "At present it would be best to consider the Olmecs as developing to a high degree certain aspects of culture which proceeded from broad general bases of Preclassic culture that were shared by many peoples, without implying a greater inventiveness, originality, antiquity or other aspects of priority to them. Our present degree of knowledge is a poor basis for dogmatic statements as to the origin of certain Classic cultures." Considering that Peterson has already stated on page 56 that the great Olmec center of La Venta flourished according to recent radiocarbon dates, between 800 and 250 B.C., these are odd statements to make about a civilization which is clearly Middle Formative, not Classic. This is only one of many signs that the manuscript must have been submitted to the publishers at a distant date, for the text has not been revised in the light of recent information.

On the credit side, there are certainly some good things in this book for which we ought to be grateful. The treatments of the Toltec period (Chapter IV, "Ebb and Flow in Mexico") and the growth of the Aztec state (Chapter V, "Empire Building in Mexico") are well organized and certainly more trustworthy than Vaillant's account of the same events. Tables I and II present in a useful form the principal deities of the Aztec and Maya pantheon, condensing the scattered information on attributes and other details for which one would otherwise have to spend a search of some hours. Chapter X, "Numbers and Time Counting," is an adequate summary of both the Mexican and Maya calendar systems; however, it should be noted that the earliest dated monument in the Maya area is not, as Peterson states (p. 196), Stela 9 at Uaxactun or even the more recently found Stela 29 at Tikal, but Stela 1, El Baul. The brief descriptions of the more important pre- and post-Conquest texts may also be of some help to the casual student.

The style is hardly what one might call felicitous, alternating a pedestrian prosaicness with misplaced informality, rather in the nature of padded-out lecture notes for a beginning course in Mesoamerican archeology. The plates, while thoroughly jumbled in the text, are excellent and often of little-known pieces, and the line-cuts are adequate.

To return to our original complaint, the lack of a good text. If only Covarrubias' work was half its price, or the archeological section in Eric Wolf's *Sons of the Shaking Earth* twice its present length, this reviewer would be quite content.

*Bibliografía Básica de Arqueología Americana.* JOSÉ ALCINA FRANCH (Ed.). (Publicaciones del Seminario de Antropología Americana, Vol. I.) Sevilla, Spain: Universidad de Sevilla, 1960. 124 pp., index. n.p.

Reviewed by RICHARD B. WOODBURY, *University of Arizona*

Bibliographies are such indispensable tools for research that the publication of an adequate list of books on New World archeology would be welcomed widely. Alcina states that he has emphasized recent works and the most useful and generally accepted older works, but the result falls short of his aim. There are 1,232 entries, a few titles necessarily appearing under more than one heading. They are arranged by areas, except for the category of "El Paleolítico Americano." The areal groupings, and the chronological subdivisions of some of them, result in too many overly brief sections, such as the Bahamas and Jamaica (6 titles), or the Atacameña area (8 titles). But more serious are the omissions that even a brief inspection will reveal. Many publications of the 1940's and 1950's, of great importance, are missing, such as Willey's report on Virú Valley settlement patterns and his symposium on New World settlement patterns, Morley's "Inscriptions of the Peten," Cole's "Kincaid," Jennings on Danger Cave, Brew on Alkali Ridge, Webb on Indian Knoll, and Giddings on the Kobuk River. In view of such omissions it is regrettable that numerous quite unimportant titles are included. About half the titles are from 1945 to 1957, almost none later than that. The volume is reproduced by an inexpensive process from typewritten copy but is only rarely illegible, and there are relatively few errors in authors' names, in titles, and in dates. A revised edition of this bibliography could be genuinely useful.

*An Introduction to Plains Apache Archeology—The Dismal River Aspect.* JAMES H. GUNNERSON. (Bureau of American Ethnology, Bulletin 173: Anthropological Papers, No. 58. Smithsonian Institution.) Washington, D. C.: United States Government Printing Office, 1960. pp. 131-260, 38 plates, 12 text figures including one line drawing map, appendices, bibliography. n.p.

Reviewed by FRANK RAYMOND SECOY, *New York*

This publication gives a comprehensive summary, up to the present date, of the archeological findings classifiable either definitely or tentatively as belonging to the Dismal River Aspect.

In the preface the author points out that the bulk of the present data was obtained from one site in south-central Nebraska and three sites in north-central Nebraska in connection with the Missouri River Basin archeological salvage program.

In the Introduction the distribution of this complex is stated to be western Nebraska, western Kansas, eastern Colorado, and southeastern Wyoming. The date is circa 1700 and it is attributed to the Plains Apache. The material culture is simple, indicating an economy based mainly on hunting with some agriculture. The sites are interpreted as semi-permanent villages or temporary camps chosen with little concern for defensibility. Evidence linking this complex to the Southwest is present, but the

author feels it is best, "... characterized as having an alien base with an overlay of Central Plains traits" (p. 141).

Next follows a brief review of most of the previous work germane to the Dismal River and its assignment to the Plains Apache.

Then the author describes the three slightly differing natural areas of the Central Plains which these people inhabited: the High Plains, the Sandhills, and the Colorado Piedmont. The following 34 pages are devoted to a full description of the archeological data from site 25HN37, otherwise known as White Cat Village. The nature of the pottery is described in detail and its importance is stated as being, "... the most diagnostic artifact of the Dismal River complex..." (p. 160). And, on page 246, it is pointed out that:

Single sherds from other complexes could be confused with Dismal River pottery, but there is little chance for confusion between series of Dismal River sherds and pottery from any other identified complex on the Central Plains. The most diagnostic traits of Dismal River pottery are its gray-black color, smooth or simple stamped surface, gritty paste, tendency toward straight square breaks, small sherd size, tempering (usually fine to medium sand or occasionally mica) and scarcity of decoration, which, when present, is almost always confined to the lips and consists of punctates and incised or impressed lines.

This particular site was dated by dendrochronology at 1723 from wood charcoal remains.

The next 28 pages (pp. 180-208) are devoted to the data from the Sandhills sites in north-central Nebraska. The sites in Lincoln County, Nebraska, are covered in pages 209-12. Pages 212-22 deal with Southwestern Nebraska, and pages 212-27 describe the sites of Western Nebraska. Eleven pages are devoted to listing and briefly describing collections from all of the sites outside of Nebraska which can be definitely or tentatively assigned to the Dismal River complex (northeastern Colorado, southeastern Colorado, southeastern Wyoming, southwestern South Dakota, and western Kansas).

The remaining 14 pages are devoted to a summary and discussion of implications of the previous data. The time span of the Dismal River Aspect is very short, with a 50 year period centered on 1700 including all the dated sites. It has no known archeological antecedents in the Plains which could be ancestral. "... Dismal River Pottery seems to present a non-Plains pottery tradition modified by the incorporation of the Plains technique of surface treatment called simple stamping" (p. 240).

The house type becomes a source of confusion when attempts are made to relate it to other complexes and cultures. On page 178, when summarizing the data from White Cat Village, the author states:

The houses at 25HN37 had five vertical center posts arranged in a nearly regular pentagon and, in some cases, two posts indicating an entrance to the east. The fireplace was in the center of the lodge, whose living area was increased by the use of poles placed beyond and leaned against the central framework. The structure probably resembled a Plains earth lodge except that it was smaller, less deeply excavated, and may not have been earth covered.

While, in the general discussion, the author states:

The Dismal River house is not the Plains earth lodge. Nowhere else in the Plains has a pattern involving five basic posts been found. In Dismal River houses the center posts form a ring with a diameter about half that of the house. This trait is characteristic of many Plains earth lodges, but it is by no means limited to the Plains. Nor are the central fireplace and the eastern entrance. It is possible that the Dismal River house represents a compromise between the Plains earth lodge and a type of dwelling known earlier to the Apache such as the Navaho hogan (one style of which has a basic pattern of three main posts plus two entrance posts). . . .



Other artifacts, ". . . peculiar to Dismal River as compared with other Plains complexes are: end scrapers with tangs or graver points, 'cigar-shaped' drills with lateral lugs, tubular pottery pipes, and much-polished bone punches" (p. 240). And, "A notable absence from the Dismal River assemblage is the Catlinite pipe, a protohistoric and historic time marker in the eastern part of the Plains" (p. 241).

Considering the relationship between the Dismal River Aspect and the culture area of the Southwest, the author points out: "Many of the artifacts from Pecos (Kidder 1932) show a striking similarity to those of the Plains in general and to those of the Dismal River Aspect in particular" (p. 241). The most striking instance is: "The types of drills are nearly duplicated in the two assemblages. This is especially interesting since the plain-shafted or 'cigar-shaped' drills and the same type of drill with lateral projections seem to be restricted in the Plains to Dismal River" (p. 242). Also, "Tubular clay pipes shaped like specimens from Pecos, but much less ornate, are found at some Dismal River sites" (p. 243).

The author suggests one further relationship. A reconsideration of the material collected from caves on Promontory Point in north-central Utah by Julian H. Steward in 1937 convinces him that, "The similarities between the two complexes are sufficiently numerous and specific to suggest that the Promontory culture is closely related to the Dismal River Aspect" (p. 244).

In conclusion, the economy of the Plains Apache was primarily based on bison hunting and secondarily on a corn-squash horticulture. A great proportion of the stone tools are skin-working tools. Stone-working was mainly by percussion and pressure flaking. There was also some grinding, pecking, and polishing. The stone artifacts show a full mastery of the techniques combined with a strictly utilitarian approach. Thus, "Well-chipped cutting edges or scraping edges occurred on tools otherwise shaped little or not at all" (p. 246).

Only five pottery vessels were restorable and these, ". . . are globular to somewhat elongated with a constricted neck" (p. 247). The largest had a maximum height and diameter of 23.5 cm. In addition, "Unmistakable painted Southwestern sherds have been found at Dismal River sites in Kansas and southern Nebraska" (p. 247). And, "Clay and occasionally stone pipes were made. Pipes were of two types. The more common were the tubular or 'cloud blower' pipes resembling those from Pecos but generally far less elaborate. The others are elbow shaped" (p. 248).

Another unique trait of the Dismal River complex are baking pits about 3 feet in diameter and in depth, ". . . used for cooking green corn and perhaps other foods in large quantities" (p. 250). In addition, "The cache pits which are characteristic of most of the Plains agricultural complexes are missing at Dismal River sites" (p. 250).

In reference to trade, and basing the evidence entirely on the archeological record, the author concludes that there is evidence for only a small amount. "They received such things as metal jingles, iron awls, and possibly axes and guns from European sources either directly or indirectly" (p. 251). Trade with Indian groups of the Southwest is evidenced by the occurrence of a few turquoise beads and the aforementioned Southwestern pottery sherds.

Appendix 1 is in table form and gives a complete classification of the sites of the Dismal River Aspect. Appendix 2, also in table form, lists, "Sites definitely or tentatively assigned to the Dismal River Aspect and the agency having information concerning them."

*The Ozark Bluff-Dwellers.* M. R. HARRINGTON. (Indian Notes and Monographs, Volume XII.) New York: Museum of the American Indian, Heye Foundation, 1960. xiii, 185 pp., bibliography, 16 figures, frontispiece, 47 plates. n.p.

*Reviewed by* ROBERT T. BRAY, *University of Missouri*

This little book contains an account of the excavation and description of the findings in 19 rockshelters in the Ozarks region of northwest Arkansas and southwest Missouri. The work, carried out in 1922 and 1923, was aimed specifically toward the recovery of ordinarily perishable remains of the prehistoric Indian. Greater success in this purpose was apparently achieved, if by anyone, only by those who preceded Mr. Harrington in the region. In keeping with the avowed limited purpose of the expeditions, only those rockshelters which were found to be dry enough to preserve various items of a perishable nature were explored. Such shelters were found along Clifty creek and along the White, Kings, and Buffalo rivers in Arkansas, and along the Elk and Cowskin rivers in Missouri.

Reports on the individual shelters are presented in narrative fashion and in the chronological order in which they were excavated. Descriptions of the shelters, features within them, and the findings are generally brief. The book, printed in Germany, is attractively done, and there are few typographical errors. The 47 full-page plates and the 16 text figures, though small, are of good quality and are well reproduced.

Important archeological remains of the sort described have been reported in the humid East from only two regions: the Ozarks and parts of Kentucky and Tennessee. Because similar undisturbed remains can probably no longer be found in either of these regions, this report is particularly valuable.

In reading this book, one becomes increasingly aware of the great wealth of vegetal materials utilized by the Ozarks Indians. The inventories of perishable items from many of the sites read rather like grocery lists of an eccentric vegetarian. The scores of identifications of seeds, nuts, grasses, fibers, stems, leaves, woods, wood bark, and the like were made by M. R. Gilmore, first director of the Ethnobotanical Laboratory at the University of Michigan, and these identifications comprise one of the lasting values of the book.

The inhabitants apparently took full advantage of wild foodstuffs and, in addition, cultivated several important species such as beans, squash, pumpkins, sunflowers, and four varieties of maize.

Insights into the wearing apparel of the people may be had from the descriptions, drawings, and photographs of such items as a breechcloth, sandals, leggings, belts, and robes or cloaks.

Their proficiency in weaving baskets, mats, and bags from grasses and split canes is rather impressively brought out. Wooden artifacts, in one instance, counterparts of what we find today in stone and bone, are illustrated and described. Only 15 chipped stone artifacts of all kinds are illustrated, and pottery, though frequently mentioned, is more often noted for its scarcity than for descriptive analysis. It is regrettable, from the present day standpoint, that more serious effort was not made to associate non-perishables, particularly projectile points and pottery, with the perishable material as it was being excavated.

Mr. Harrington states, on page 107 and pages 124-25, that at least some of the storage pits, in which occurred the great bulk of the fine perishable materials, belonged to the post-Bluff Dweller occupation. He makes no distinction, however, between the contents of the pits which occurred in the different occupational zones, where these

could be recognized, and does not discuss the notion that these perishables might belong, for the most part, rather than in minor part, to the post-Bluff Dweller occupation.

Mr. Harrington's conclusions concerning the Bluff Dweller and post-Bluff Dweller are much the same as those he made 36 years ago. There is no effort to fit the material into any current terminological framework. Bluff Dweller is compared, less explicitly than in 1924, with Basketmaker III and, apparently as a more recent interpretation, with the "Caddoan mound builders." Evidently he believes there was no relationship between Bluff Dweller and post-Bluff Dweller which he tentatively equates, as in 1924, with "Kansa or Osage."

Mr. Harrington might have found it pertinent, in his interpretations, to take advantage of the recent work published by Chapman and Bray on the area, and by Baerreis, Bell, et al., on the northeast Oklahoma area. He might then have found it difficult not to see how favorably the dry shelter stonework fits into the superpositional sequence defined from the nondry shelters in the same area.

*Oncota Sites on the Upper Iowa River.* MILDRED MOTT WEDEL. (The Missouri Archaeologist, Vol. 21, Nos. 2-4.) Columbia, Missouri: The Missouri Archaeologist, 1959. viii, 181 pp., bibliography, cover plate, 36 figures, 4 maps, 2 tables. n.p.

Reviewed by MARVIN F. KIVETT, *Nebraska State Historical Society*

This report describes archeological investigations carried on at seven sites in 1934 and 1936 by the Iowa Archeological Survey. These sites were on or near the Upper Iowa River in northwestern Iowa and this report deals with the Oncota materials from these sites. Excavations were carried on under the direction of Dr. Charles Reuben Keyes, State Archeologist, with Mr. Ellison Orr serving as field director. A labor crew was provided by various forms of the national emergency relief work acts in force at the time.

In Iowa, as in many other states, adequate provisions were not made for the immediate analysis of the collections and preparation of the final report. This report is an excellent example of the important information available to research workers as a result of this emergency work.

The important Oncota complex, although the subject of considerable field work and discussion, had not been adequately described or defined. Such final definition could not be made until the basic site reports were available for detailed comparisons. This report provides the detailed inventories for the seven sites and a discussion and consideration of the Orr focus.

*Table Rock Pueblo, Arizona.* PAUL S. MARTIN and JOHN B. RINALDO. (Fieldiana: Anthropology, Vol. 51, No. 2.) Chicago: Chicago Natural History Museum, 1960. Pp. 131-298, bibliography, frontispiece, 91 illustrations (including maps and photographs), 2 tables. \$5.50.

Reviewed by JESSE D. JENNINGS, *University of Utah*

As pointed out by Woodbury in his review (*AMERICAN ANTHROPOLOGIST*, 62:1106) of the first number in this volume, Martin and Rinaldo have essentially closed off their study in the heartland of the Mogollon culture near Reserve. They are pursuing a logical development in their long range study of the Mogollon culture by attempting to determine the sequence of culture and events which overtook the Mogollon people after the 14th century when the story in the Reserve area pinches out.

The volume here being reviewed is another chapter in their new study. It is systematic and logical in its development, as are the separate chapters in their studies of

the past 15 years. Space does not permit any effort at systematic analysis of this fine report. We simply quote one of the most important "conjectures" appended to the summary section, which is: "Several traits suggest that our previous conjectures concerning a relationship between the Mogollon and the Zuni or Hopi (Western Pueblo complex groups) have some foundation in reality." The authors are also able to say that regardless of the final ethnographic identification of the Tablerock Culture, as they have called it provisionally, its antecedents were indeed Mogollon. In such pottery types as Reserve and Tularosa Black-on-White, the authors perceive a link with their work in the Reserve-Pinelawn areas, especially at Foote Canyon Pueblo. In the same collections are found Zuni Glaze types and Hopi Buff pottery, along with such traits as the paved kiva and the cluster of rooms embracing a kiva in a plaza type layout.

In short, Martin and Rinaldo quite clearly expect to tie Zuni and Hopi material culture and architecture to the earlier Mogollon sequence. This has often been postulated and, I presume, is one of the basic assumptions in the Southwest. Nonetheless, only through the work of Rinaldo and Martin will this be demonstrated as fact rather than highly reasonable speculation. The date for Tablerock Pueblo is in the neighborhood of A.D. 1350. This date results from the dendrochronological analysis of a piñon log and a radiocarbon determination upon the same log. The tree-ring date is given as A.D. 1331 with a few missing rings, the radiocarbon date from the Groningen Laboratory is A.D. 1345  $\pm$  50 years. This is an unusually close correlation of these two dating techniques and serves to reinforce one's wavering trust in the radiocarbon method.

*The Hubbard Site and Other Tri-Wall Structures in New Mexico and Colorado.* R. GORDON VIVIAN. (Archeological Research Series, Number 5.) Washington: National Park Service, U. S. Department of the Interior, 1959. vii, 92 pp., bibliography, frontispiece, 63 illustrations, 21 tables. \$1.00.

*Reviewed by* JESSE D. JENNINGS, *University of Utah*

As the title implies, the Hubbard Site at Aztec Ruins National Monument is an enigmatic tri-walled structure, which is only one of several such Anasazi structures. As an effort at improving, or rounding out, the interpretive story at Aztec Ruins National Monument, the excavation of the Hubbard Site was undertaken in 1950. For various reasons it was not completed at that time. Several people worked on the project intermittently, including Gordon Vivian, Leland Abel, T. B. Onstott, and Tom Matthews. The present volume is a report of the findings at the Hubbard Site itself, as gleaned by Vivian from the not always coordinated notes of the several investigators. Additionally, and this perhaps is its greatest value to general Southwesterners, the book brings together information about, and references to, the several tri-walled structures of the Anasazi area. These of course are of primary interest to students of the Southwest and Mesoamerica. The report also includes the only description of the tri-walled structure at Pueblo del Arroyo in Chaco Canyon; although this was partially excavated in 1926 by the National Geographic Society's Pueblo Bonito Expedition, the excavation was finally finished by the National Park Service in what is called a salvage operation.

In his consideration of the problem of the function of the tri-walled structures, and the possibility that these are evidence of strong Mesoamerican cult influences on the Anasazi area, Vivian essentially rejects the notion of Mesoamerican influence, preferring the idea that the tri-walled structures are little more than elaborations of the indigenous kiva. Aside from the description of the somewhat complicated excavations at the Hubbard site and the summarizing of the tri-walled structures, the other contribution of this publication lies in the careful analysis of McElmo, Mancos, Chaco, and Puerco pottery assemblages.

*Cultural Sequences at The Dalles, Oregon.* L. S. CRESSMAN. In collaboration with DAVID L. COLE, WILBUR A. DAVIS, THOMAS M. NEWMAN and DANIEL J. SCHEANS. (Transactions of the American Philosophical Society held at Philadelphia for promoting useful knowledge.) (New series, Vol. 50, part 10.) Philadelphia: The American Philosophical Society, 1960. 107 pp., appendices, 2 charts, 61 figures, index, 3 maps attached. \$3.00.

Reviewed by IRVING ROUSE, *Yale University*

This is the latest contribution in one of the most intensive and sustained programs of archeological research in the Americas. Cressman began to excavate in the state of Oregon during the early 1930's, concentrating on the southeastern quarter of the state, which forms part of the Great Basin (see his *Archaeological Researches in the Northern Great Basin*, Carnegie Institute of Washington Publication 538, 1942). Subsequently, he moved to the Klamath Lake region, in the south central part of the state, just east of the Cascade Mountains (reported in *Klamath Prehistory*, Transactions of the American Philosophical Society 46: pt.4, 1956). The present monograph is concerned with the Dalles region, in the north central part of the state, where the Columbia River passes through the Cascades. Cressman is now working on the coastal region west of the Cascades, and we may expect an additional monograph on this area at a later date.

As a result of his original research, Cressman successfully carried the record of man in the southeastern part of the state back to about 10,000 B.C. and correlated it with changes in the environment since that time, especially with the great drought of the Altithermal period, ca. 6000-2000 B.C. (1942:94). His findings led him to divide the Indian occupation preceding the drought into "two provinces, roughly contemporaneous but markedly different in cultural characteristics": (1) the Great Plains east of the Rockies, with its big-game hunters, e.g., of the Folsom culture; and (2) the Great Basin, occupied by gatherers and hunters of smaller, though also now extinct, game, who had basketry, sandals, and less distinctive projectile points (1942:1). It is (2) which Jesse D. Jennings, Norbeck, and others have recently conceptualized as the "Desert culture" and which many archeologists now believe to be basic to subsequent cultural developments west of the Continental Divide (see, e.g., *American Antiquity* 21:1-11, 1955).

Cressman's second series of excavations, in the Klamath Lake region, served to demonstrate that the original Desert culture had developed two different adaptations, one to the dry-cave ecology of southeastern Oregon and the other to the lake and marsh habitat of the Klamath area (1956:377). Again, he was pioneering; Clement L. Meighan has more recently elaborated upon this point by distinguishing three variations on the "basic seed gatherer tradition": "... hunters," "specialized lake dwellers," and "a coastal desert tradition" (*Varieties of Prehistoric Cultures in the Great Basin Region*, *Masterkey* 33:46-59, 1959).

The excavations which are reported in the volume under review introduce an additional ecological variant on the original Desert culture, that of fishermen living along the Columbia River in an environment which was especially suited to this means of subsistence. Cressman has excavated two sites of fishing peoples in the Dalles region, which contained three periods of occupation: Early, divided into three parts; Transitional; and Late, likewise divided into three parts. There are radiocarbon dates and other evidences to indicate that the Early period goes back to 8000 B.C. and that the Late period extended into historic time. The Early period is characterized by a unique complex of bone artifacts which bears, as Cressman points out (p. 41), a striking resemblance to the contemporaneous fishing cultures of the Mesolithic in northern Europe. It is not until the Late period that one finds the variety of chipped stone projectile points and stone carvings which are classic for the Plateau area.

Cressman frankly states that his approach to the archeology of the Dalles region is conditioned by his previous research: "In the Great Basin I was impressed with the importance of the relation between man and his environment both at a given period and through time. The evidence of significant changes in climate in the Great Basin forced me to think in terms of dynamic rather than classificatory relationships" (p. 72). As a result, the reader who prefers to have his archeology presented in terms of cultural phases rather than trends will not find them in this monograph, although the data needed to formulate them is there.

In a concluding section, Cressman speculates that the great drought of Altithermal time and the final retreat of the glaciers farther north may have combined to cause man and/or elements of his culture to spread westward to the North Pacific Coast and northward through the Plateau into the Arctic area; and he suggests possible linguistic correlations with these movements. His speculations should be useful as a guide to further research, and it will be interesting to see whether they are confirmed by Cressman's own work on the Oregon coast.

#### LINGUISTICS

*Explorations in Communication: An Anthology.* EDMUND CARPENTER and MARSHALL MCLUHAN (Eds.). Boston: Beacon Press, 1960. xii, 210 pp. \$4.00.

Reviewed by HERBERT LANDAR, *Los Angeles State College*

It has been remarked that nowadays anthologies are mostly either documentary boarding houses kept by Aristotelian historians, or overtenanted resorts of ill repute at the distal end of respectability. In terms of this dichotomy, it is difficult to place *Explorations in Communication*, for its editors neither annotate knowingly for the few, nor marshal cheaply for the many. They offer, rather, 24 relatively short essays "from *Explorations*, a journal on communications published between 1953 and 1959. All its issues are now rare collectors' items" (p. ix).

These are, in a manner of speaking, essays of quality and appeal; the quality is uneven, however, and the appeal will vary inevitably from essay to essay, from feuilleton to fenestella, according to the reader's wit and taste.

The pilgrims of the anthology, along with the hosts McLuhan (four contributions), Carpenter (one contribution), and Carpenter and McLuhan (one contribution), form a distinguished company, as subtle and diverse as any set of Chaucerian personae. Under a single roof we have Ray L. Birdwhistell, H. J. Chaytor, Arthur Gibson, S. Giedion, Stephen Gilman, Stanley Edgar Hyman, W. R. Rodgers, Jean Shepherd, Jacqueline Tyrwhitt, Fernand Léger, David Riesman, Robert Graves, Gilbert Seldes, D. T. Suzuki, Northrop Frye, Lawrence K. Frank, and Dorothy Lee. If we have the Zen authority D. T. Suzuki reciting haiku and telling us, "If anything is a symbol, everything is also equally a symbol, thus putting a stop to symbolism" (p. 42), we also have the critic Northrop Frye arguing that some poetry is best understood partly in terms of traditional symbols or archetypes of multiple implication. And if we have the psychologist Lawrence K. Frank reminding us that the incest taboo and masturbation are formed upon "tactile communication" (pp. 10-11), we also have the anthropologist Dorothy Lee reminding us that the Wintu deer merges into deerness as its name fades upon Wintu lips (pp. 14-15), or that temporality is meaningless in the translocation of a Trobriand *yowana* by a sprout-marked *silasata* (p. 141). Robert Graves, in "Comments on 'Lineal and Nonlinear Codifications of Reality,'" has nothing to say about that, though he does point out the remoteness of the possibility that Malinowski "has

hideously deceived us with his Legend of *Inuvayla'u*, too bawdy a story to repeat here, which has perfect development. . . . And there is one side of island life in which, according to Malinowski, the Trobriander can be regarded as extremely purposive: the erotic side" (p. 161). There is, in short, an entertaining diversity of opinion, of wide appeal, in the editorial selection.

Some anthropologists will be grateful for the inclusion of Birdwhistell's "Kinesics and Communication" (pp. 54-64). Defining the concern of kinesics as "the visual aspects of non-verbal, inter-personal communication" (p. 54), Birdwhistell mentions as areas of study pre-kinesics, micro-kinesics, and social kinesics. The least particle of isolatable body motion, e.g. of a person's eyelids, is a kine. Kines with differential meaning, e.g., the abstracted composite of droopy eyelids and bilaterally raised brows having depressed median portions, as contrasted with droopy eyelids and a low unilateral brow lift, are classed into kinemorphs. A central aim of the student of kinesics is to understand how kinemorphs are related to linguistic and paralinguistic elements, to understand "the relationship between visible and audible communication" (p. 59). Birdwhistell's sketch caresses but does not satiate the great expectations spurred by the title of this generally pleasant, if sometimes not properly serious, assemblage.

*Linguistic Diversity in South Asia: Studies in Regional, Social and Functional Variation.*

CHARLES A. FERGUSON and JOHN J. GUMPERZ (Eds.). (International Journal of American Linguistics, Volume 26, Number 3.) (Indiana University Research Center in Anthropology, Folklore, and Linguistics, Publication Number 13.) Bloomington: Indiana University Press, 1960. viii, 118 pp. \$3.50.

*Reviewed by H. A. GLEASON, JR., Hartford Seminary Foundation*

One observer might look at certain data and label it as the "social dimensions of language use," another as the "linguistic dimension of social difference." But from whichever side it is viewed, it is certainly one of the most sadly neglected problems in anthropology. This is most unfortunate, for the subject holds out great promise of intrinsic interest and practical importance and may make important contributions to our understanding both of language and of social dynamics.

The volume under review contains several contributions bearing in one way or another on this problem. It is an outgrowth of a symposium "Language and Culture Dynamics in South Asia" held at the annual meeting of the American Anthropological Association in 1957.

M. Shanmugam Pillai ("Tamil—Literary and Colloquial") describes the differences between two styles of speaking employed by the author. The list is extensive, and the differences are often surprisingly great, but there is little more than a list.

Two of the contributions deal with Kannada. William Bright ("Linguistic Change in Some Indian Caste Dialects") first delineates some of the differences in the speech of urban Brahmans and rural Okaligas and then shows that these differences can be explained in part by different courses of linguistic change in the two speech communities. One accepts loans readily but is resistant to phonemic change; the other is rather conservative in accepting new items, but more innovating in change within the system. This difference is thought to be correlated with the Brahman contact with Sanskrit. Cause aside, it is another demonstration that it is too simple merely to label languages or dialects as "conservative" or "innovating." William McCormack ("Social Dialects in Dharwar Kannada") reports on differences actually found in tape recordings of Brahman, Lingayat, and Harijan speech. The paper makes its chief contribution in

showing something of the complexity of the differences and their interaction with personal, educational, and situational factors.

Two papers deal with Bengali. Edward C. Dimock ("Literary and Colloquial Bengali in Modern Bengali Prose") traces the complex history that has led to two competing literary languages being used side by side in Modern Bengali. Munier Chowdhury ("The Language Problem in East Pakistan") describes the further complications which result from the separation of a large section of the Bengali speech community from the old cultural center at Calcutta. This has broken the old equilibrium between the local vernaculars and the two literary dialects and has led to the emergence of a new form intermediate between the vernaculars and Standard Colloquial Bengali. An educated person in East Pakistan now "has four instead of three models of Bengali to juggle with."

John J. Gumperz and C. M. Naim ("Formal and Informal Standards in the Hindi Regional Language Area") formulate the linguistic situation in terms of three language strata, each complex. One is a vast complex of intergrading, but widely diverse, local dialects. Another is not strongly differentiated geographically but is a complex of competing styles, writing systems, loan word sources, etc. The third is an intermediate system of regional standards. A typical individual must operate in reference to at least one (often several) forms from each stratum. Social situations and differing purposes determine the choice. The authors go on to show something of the diversity in the Hindi-Urdu stratum by comparing the phonologies and lexica of the two chief styles and noting something of the variation within each.

Finally, Charles A. Ferguson and John J. Gumperz have written an introduction which attempts to present a general framework for understanding of the linguistic diversity which is described in the following papers.

Taken as a whole, I feel that the symposium is a bit above the average for such collections. There is at least some semblance of a unifying theme running through the contributions. The introduction makes a valiant attempt at providing a general framework. The material is mostly quite pertinent. The greatest shortcoming is that competence in assessing the linguistic facts is not always matched by equal competence in assessing the social dimensions. As a result, too much is left hanging, not adequately related to the symposium topic. A little more anthropological skill or awareness might have shown more directly the bearing on social dynamics. Nevertheless, the papers do give an introduction to the subject. They show something of the magnitude of the differences, a magnitude that few Americans would imagine possible.

The papers point up the potential significance of further work along those lines. South Asia is a crucial area for such research. It is in a sense a type area, a region in which the problems are thrown into high relief and made particularly challenging for investigation. But the problems are by no means unique—all of the components are widely spread over the world. What can be learned in South Asia will be of fundamental importance for understanding problems of world-wide import. Let us hope that this symposium may serve as a challenge to more work of the same sort.

*Vocabulario Tarahumara*. K. SIMON HILTON et al. (Serie de Vocabularios Indígenas Mariano Silva y Aceves, Núm. 1.) Mexico, D.F.: Instituto Linguístico de Verano, 1959. 216 pp. n.p.

*Vocabulario Cora*. AMBROSIO McMAHAN Y MARIA AITON DE McMAHAN. (Serie de Vocabularios Indígenas Mariano Silva y Aceves, Núm. 2.) Mexico, D.F.: Instituto Linguístico de Verano, 1959. 193 pp. n.p.



*Vocabulario Zapoteco del Istmo.* VELMA PICKETT et al. (Serie de Vocabularios Indígenas Mariano Silva y Aceves, Núm. 3.) Mexico, D.F.: Instituto Lingüístico de Verano, 1959. 246 pp. n.p.

*Vocabulario Popoluca de Sayula.* LORENZO CLARK Y NANCY DAVIS DE CLARK. (Serie de Vocabularios Indígenas Mariano Silva y Aceves, Núm. 4.) Mexico, D.F.: Instituto Lingüístico de Verano, 1960. 165 pp. n.p.

Reviewed by C. F. VOEGELIN, *Indiana University*

Now that the Wycliffe Bible Translators, better known as the Summer Institute of Linguistics (SIL), have increased the scope of their work to include the publication of dictionaries, under the able leadership of Ben Elson, the available lexical sample of the world's languages will be greatly increased. We are hopeful that the accumulated work on dictionaries will serve as a better basis for systematizing semantics than the grammatical basis upon which Boas built. (The best review article on the Boas basis of semantics is Dell Hymes' in *Anthropological Linguistics* 3:22-54.) The first impact that SIL had on the whole field of linguistics was to provide data making possible worldwide generalizations in phonological typology; it is fair to say that Hockett's *Manual of Phonology* was dependent for its scope as much on the wide variety of phonemic systems, published largely in *IJAL* and written largely by SIL workers trained by Pike, as it was on previous theoretical considerations in typology. Some future manual of lexical resources in languages of the world may well be analogously dependent on publications like the following, when multiplied a hundred fold.

Vocabularios Indígenas 1 (Tarahumara) begins with eight pages of grammatical notes (morphology), and indexes the lexical items in Part 1 (Tarahumara—Español) to the grammar. These entries cover pages 3-126—a couple of thousand altogether, with occasional pictures, as of a rabbit, drawn where space permits beside word lists. Part 2 (Español—Tarahumara) is interesting in more than one way—e.g., it translates Spanish phrases into single Tarahumara words and shows that entirely different Tarahumara words are often called for where Spanish paired phrases show parallelism at all points but one.

Vocabularios Indígenas 2 (Cora) begins with ten pages of coded morphology, but the word lists which follow are in general not indexed to the morphology. Though this Cora dictionary is about the same size as the Tarahumara, and both languages belong to the Uto-Aztecan family, the order of presentation is reversed, Spanish—Cora preceding Cora—Spanish. And the second part is merely a list of Cora words matched with Spanish gloss.

Vocabularios Indígenas 3 (Zapotec) is also in two parts, but followed by appendices on the phonetic value of the transcription, on dialect variability, on stress, on tone, and on a concise introduction to morphology to which the lexical items are indexed.

Vocabularios Indígenas 4 (Popoluca) gives about 1500 entries (pp. 1-78) which quite often do more than gloss a Popoluca word with a Spanish, as that for bean (*frijol*). This entry is followed by three entries in which the Popoluca morpheme for bean is the first constituent of words meaning *milpa de frijoles*, *caldo de frijoles*, and *tortilla de frijoles*; and by a fourth entry in which the morpheme for bean is noninitial—hence failing to be in alphabetic order but succeeding in a more interesting design, that of relating the morpheme for bean to the cultural domain in which it fits, in the Popoluca view. Alphabetic order is the curse of dictionary making. Such order is strictly followed in the Spanish—Popoluca part (pp. 79-147). This little book, with interest-arousing pictures of animals, plants, and artifacts in both its lexical parts—rather lively drawings

—ends with grammatical notes (pp. 149–65) which are neither lively, nor interesting, nor keyed into the preceding lexical parts.

Dictionaries of the size and scope of the above sample have their chief usefulness in making and controlling translations. All those in this sample and all those planned for it, presumably, are bilingual dictionaries. All include a grammatical sketch but lack prefatory or appendical essays on the lexical problems peculiar to a particular language. That each language contains lexical problems peculiar to itself was one of the chief lessons learned at the November, 1960, Conference on Lexicography where a half dozen working papers were devoted to an exposition of such peculiar problems in Thai (by Mary Haas), in Turkish (by Andreas Tietze), in Japanese (by Samuel E. Martin), in Pashto (by Herbert Penzl), in Arabic (by Richard S. Harrell), and in Modern Greek (by the Kahanes)—all in respect to bilingual dictionaries which facilitate translation.

Without impugning the importance and necessity of the translation aspect of lexical work (which appears to be the chief aspect so far emphasized by the Wycliffe Bible Translators), there can be little doubt that another aspect of lexical work turned out to be much more interesting at last November's Conference on Lexicography. For want of a better term, the latter may be called the "semantic" or "typological" aspect of dictionary making; and a half dozen working papers were also devoted to this aspect at the Conference. Harold C. Conklin's on lexicographical treatment of folk taxonomies included a highly sophisticated bibliography which, if followed by the Wycliffe Bible Translators, would surely have an enormous impact and add new dimensions to their future dictionaries, to the enrichment of this field of lexical work which, though still in need of plowing, is no longer taboo. Promising and liberating also were other Conference papers, by Yakov Malkiel, by Allan Walker Read, by H. A. Gleason, Jr., by H. M. Hoenigswald, and by Uriel Weinreich. Semantic investigations through lexical work can be more widely applied to field work by the Wycliffe Bible Translators than by any other group. We hope they will not content themselves in future with an exclusive concern with the translation aspect, as in the initial sample reviewed here; but, in addition, press forward their own relevant interests, and thereby enrich ours, by giving equal weight to the semantic and typological aspect of dictionary making.

*Yana Dictionary.* EDWARD SAPIR and MORRIS SWADESH. Edited by MARY R. HAAS. (University of California Publications in Linguistics, Vol. 22.) Berkeley and Los Angeles: University of California Press, 1960. xi, 267 pp., appendix. \$5.00.

Reviewed by A. RICHARD DIEBOLD, JR., *Yale University*

Edward Sapir's workmanship was such that on many occasions since his death his manuscript materials have been edited and made available through publication, showing again and again the esteem his work enjoys among scholars today: Sapir, being in advance of his times, is still not dated. The posthumous reworking of his *Yana* lexicographic materials by Swadesh is another welcome addition. The work is especially welcome in view of Sapir's sizable investment in time and effort with the investigation of *Yana*; although much of his *Yana* research was published, important parts remained in note form, largely unavailable to other investigators. The particular history of Sapir's *Yana* research is outlined briefly by A. L. Kroeber in the Preface, and in more detail in the Introduction written by Swadesh. It is easily seen that the present *Yana Dictionary* by Sapir and Swadesh is a capstone to a program of research which had its inception more than half a century ago, and which occupied Sapir on and off throughout his life. It makes infinitely more useful such descriptive materials as exist on *Yana* and provides

a valuable tool with which to carry on the comparative Hokan studies initiated by Dixon, Kroeber, and Sapir. It is in the latter capacity that the dictionary will probably find its greatest employment.

The Preface by Kroeber is followed by an Editor's Note in which Haas describes the considerable revision and expansion carried out on the original Sapir-Swadesh manuscript. The expansion principally involves the inclusion of an English-Yana section (the original was only Yana-English). The revision includes realphabetization; both changes were undertaken to facilitate the dictionary's value as a comparative source. Furthermore, vocabulary from several other sources (all collected by Sapir) were included in the revision.

Swadesh has written an excellent introduction to the dictionary in which he gives an outline of Yana phonology and morphology. Dialects are discussed, since the dictionary purports to include materials from all of three Yana dialects, Northern, Central and Yahi. A small number of Southern forms are also included. There are comments on the genetic pedigree of Yana within the Hokan stock.

The main body of the work, the Yana-English section, is impressive. Swadesh asserts that "the aim of the dictionary entry is to give in succinct form the full data (or as much as is known) of the phonetic form of each element, its meaning, the dialects in which it occurs, the variations of form and meaning according to dialect, special facts about positions and combinations in which the element occurs, etymology, and examples." The task sounds rather formidable, but is indeed well-realized. The reader will want from neither meagerness in the number of entries nor for examples of the various cited forms in context. Summary: a piece of high-quality workmanship.

*Central Sierra Miwok Dictionary with Texts.* L. S. FREELAND and SYLVIA M. BROADBENT. (University of California Publications in Linguistics, Vol. 23.) Berkeley and Los Angeles: University of California Press, 1960. vii, 71 pp. \$1.75.

*Reviewed by A. RICHARD DIEBOLD, JR., Yale University*

It is a perverse development in American Indian linguistics that, while an investigator may analyze and describe a language in great detail, his investigations seldom realize publication save in a piecemeal fashion. Too often, the part left in manuscript is the lexicon. The Survey of California Indian Languages, through the University of California Publications in Linguistics, has happily reversed this situation in a number of instances; one such is the present *Central Sierra Miwok Dictionary* by Freeland and Broadbent. In this case the junior author is responsible for compiling a Central Sierra Miwok-English and English-Central Sierra Miwok dictionary from the manuscript materials of the senior author, Freeland, who had earlier published a Miwok grammar. There are, apart from the dictionary, 16 pages of Central Sierra Miwok texts collected by Freeland and compiled by Broadbent. These may be of interest because of their ethnographic content, but their relation to the dictionary (without an outline of structure and interlinear translations) is more than a bit obscure.

The entries are listed alphabetically. For each there is a reference to the appearance of the form in either the Freeland grammar or Freeland's notes. And each item is categorized as to its form-class. Apart from the misuse and abuse of linguistic zero ( $\phi$ ) as a category of entries (surely these forms (?) are an aspect of morphophonemics or grammar), the reviewer was favorably inclined to the dictionary. It will be a valuable tool to the comparativist and others.

## PHYSICAL ANTHROPOLOGY

*Evolution Above the Species Level.* BERNHARD RENSCH. New York: Columbia University Press, 1960. xvii, 490 pp., bibliography, 113 figures, 2 indices, 21 tables. \$10.00.

Reviewed by G. E. HUTCHINSON, *Yale University*

Professor Bernhard Rensch is perhaps the most learned living zoologist in the classical tradition. In this book, which represents a revision of a second edition of a German work, he has given us a volume that is of great importance for two reasons. Firstly, it presents a sensible and convincing view, with an enormous amount of illustrative detail, of the present position of evolutionary theory in a way unfamiliar to most American readers. Secondly, it may well constitute a foundation on which a very impressive building might arise in the future.

*Evolution Above the Species Level* actually begins with a discussion of intraspecific evolution, for Rensch's first theme is that the processes involved in the production of major groups are not different from those involved in subspecific and specific differentiation. Every phylum was a species once. In dealing with evolution at or below the species level, Rensch on the whole adopts the present day position that geographic isolation is of paramount importance; he was in fact one of the major proponents of this idea over thirty years ago. It is therefore interesting and important to notice that he is far from being dogmatic that geographical isolation is always involved in the increase in number of species.

The subject of transspecific evolution is introduced in chapter 4 in which the diversity of adaptation in many lines is contrasted with the apparent directional evolution in others. Along with most modern biologists Rensch believes that the evolving line is opportunistic within its capacities and is not unfolding a pre-existing program, as some orthogeneticists have held. Nevertheless, the properties of both the material universe in general and of living organisms in particular set obvious conditions on evolution and also determine more subtly the probable most effective way of solving an evolutionary problem. It is therefore usually possible to detect similarities in the evolutionary histories of related or ecologically analogous lines, and so to draw up a list of rules of kladogenesis or phyletic branching. More than a third of Rensch's book is taken up in discussing such rules. Of these rules, which have the logical status of tendency or probability statements, one known generally as Cope's Rule, namely, that there is a high probability that successive species in a line will increase in size, has a quite peculiar importance. It is important to bear in mind what we should mean by our statement of the rule. What probably happens whenever a considerable amount of kladogenesis takes place is that in the different resulting lines size changes, generally, though not always, increasing, though at different rates in different lines. Some of those becoming large most rapidly often become extinct and later may be replaced by other large forms derived from slower evolving lines. The statement of the rule is therefore that the distribution of size change in kladogenesis tends to be skewed towards increasing size. It is unfortunate that in no very good case can a real distribution be described quantitatively, though experience makes it practically certain that the rule, in this form, must apply to a great many organisms.

The enormous importance of Cope's Rule—entries in the index referring to the rule are exceeded only by the related matters of "giant animal" and "brain"—is due to the fact that any change in size is apt to imply a change in proportions, partly for mechan-

ical, partly for more subtle reasons. In the ontogeny and evolution of vertebrates, an increase in size involves an absolute increase, but relative decrease, in brain size. The size of the nerve cells increases in this case with the increasing absolute size of brain, but this probably means that they can each have more intercellular dendritic connections in a large animal than a small. Much of Rensch's long middle chapter on kladogenesis is devoted to a unique discussion of this type of material, incorporating original results from his laboratory based on a vast diversity of animals from all over the world. In final chapters the problems of progressive evolution and the evolution of consciousness are considered. The very difficult problems, pseudoproblems to some, involved in consciousness are considered seriously from a hylopsychic standpoint. The factual material on the evolution of certain kinds of behavior is mustered in Rensch's usual fascinating way. Some may not find illumination in his philosophical position, which follows from Spinoza through Fechner and Ziehen, to name only three illustrious philosophers. It is at least healthy to find that Rensch is one of the few modern scientific writers who is not afraid of taking an anthropomorphic view of man.

The importance of this book is, as has been said, twofold. It brings together, from a coherent point of view, a vast mass of information. It then seems to imply "Go on from here and see what can be discovered." This is very timely. It is likely that the decade of the 1960's will see an unprecedented development of the study of transspecific evolution, because for the first time it should be possible, by means of radiogeochemical techniques, such as argon-potassium dating, to obtain a valid, detailed time scale, at least for significant parts of the Tertiary. This should make evolutionary paleontology into a predictive science, allowing the testing of alternative hypotheses. For example, if, as has so often been supposed, many large and grotesque structures, often called hypertelic, have resulted from positive phylogenetic allometry, as size increased according to Cope's Rule, we should expect the increase in size to have less and less selective value in view of the inconvenience of horns and bosses growing disproportionately larger. This at least has often been supposed. As Rensch says, all "such hypertelic structures must be regarded as allometric by-products of increased body size, which were tolerable as long as they did not reach harmful dimensions or prove unfavorable." If this is really the whole explanation, any line exhibiting evolution of this sort should evolve at a decreasing rate approaching asymptotically to a condition in which the advantage of increased body size is balanced by the disadvantage of excessive development of hypertelic structures. If in good lineages with a real absolute time-scale this asymptotic type of evolution is found not to occur, we should have to erect other hypotheses. A number of other related problems will become accessible, and some of these involve concepts of considerable depth and generality. What really sets the pace of evolution in any actual case is, in fact, largely unknown. It is almost certain that when we have not only a quantitative, but a geochronometrically detailed and reliable paleontology, properly integrated with environmental studies by the use of sound ecological theory, there will be a number of surprises. Whatever the results, this new paleontology will bear greatly on the evolution of man. The fortunate young workers who want to develop this potential new field cannot do better than to study Rensch's book in all its elaborate detail, and then let their imaginations range over the facts.

The book is well produced and well translated, though the verbally particular naturalist will be worried by St. Hilda for St. Kilda and by occasional confusion between butterflies and moths.

*The Processes of Ongoing Human Evolution*. GABRIEL W. LASKER (Ed.). Detroit: Wayne State University Press, 1960. 108 pp., bibliographies, figures, tables. \$3.75.

Reviewed by STANLEY M. GARN, *Fels Research Institute*

The fact that man is still evolving now can be listed along with the splittable atom, as one of the major realizations of our time. At the end of a century dominated by the notion of evolution completed, this turn of thought has come about with explosive force. In fact, within a 6 month period, there were three symposia dedicated to ongoing evolution in man; one in Mexico City, the next at Bethesda, and the third at the Fels Research Institute.

The present book (a hard-cover edition of the February, 1960, issue of *Human Biology*) represents the Mexico City symposium. It is of particular interest to anthropologists because it was given at the American Anthropological Association meeting, because all but one of the six participants are anthropologists, and because it relates to the phenomena called "culture." It is uneven, as symposia tend to be when viewed in cold print, but history is always uneven in the making. Particularly to meta-physical anthropologists, this volume constitutes a picture window into a kind of physical anthropology that is post-Hooton and definitively post-Hrdlička in execution.

Certain of the papers are along relatively familiar lines. Baker's discussion, entitled "Climate, Culture and Evolution," has its antecedents in Coon, Garn and Birdsell, and papers of Marshall T. Newman, though based upon much more extensively experimental studies. Hulse's essay, "Adaptation, Selection and Plasticity in Ongoing Human Evolution," extends his previous theoretical and experimental contributions in *Human Biology* and other journals. Lasker's "Migration, Isolation and Ongoing Human Evolution" is a logical extension of earlier reports in the *Southwestern Journal of Anthropology*. Livingstone's "Natural Selection, Disease and Ongoing Human Evolution" will recall his doctoral thesis, brilliantly summarized in this journal.

Not to slight these contributors, the paper of Arno G. Motulsky deserves to be read with especial care and great appreciation by all anthropologists. It serves to show, once and for all, how culture, while minimizing certain directions of natural selection, augments the selective intensities of others. It summarizes for anthropological audiences the implications of the abnormal hemoglobins, introduces the important new area of Primaquine sensitivities, and brings into perspective the field of biochemical polymorphisms of which we are hearing more and more. I wish that I had been able to secure it for my *Readings on Race*, for Motulsky's "Metabolic Polymorphisms and the Role of Infectious Diseases in Human Evolution" is an introduction to the absorbing new field of geographical medicine.

In this collection, Earle Reynolds' "Irradiation and Human Evolution" fails to find an appropriate adaptive niche. It is a readable essay, footnoted to the teeth, and the exact antithesis of a potboiler. A century hence it may be regarded otherwise, but as of now the impact of man-produced radiation on ongoing evolution is by no means clear. In fact, the belligerent atom may have served a good purpose by alerting us to fluoroscopes in shoestores, needless pelvic X-rays in pregnancy, unrewarding clinical fluoroscopy, and 30 r full-mouth series, thus helping to minimize our load of mutations.

*The Processes of Ongoing Human Evolution*, like the *Proceedings of the Conference on Genetic Polymorphisms*, should become familiar to anthropologists in general. The one theme that unites all of the anthropologies is evolution. Evolution, in turn, is neither more nor less than the summation of natural events that directed genetic change

in the past, and directs it still. The brass ring has come around again, affording the students of culture and the students of physical evolution the opportunity to pool their knowledge and skills, retrospectively to much of our past, and prospectively toward the future of man.

*The Processes of Ongoing Human Evolution* is not one of the great scholarly works characteristic of the many Darwin Centennial volumes, but it represents research in progress. What it lacks in professional polish, and Great Names, it gains in the possession of relatively young, energetic investigators, whose future contributions will add to the historical value of this important and distinctly modest documentation of the unending evolution of our species.

*A Handbook of Anthropometry*. M. F. ASHLEY MONTAGU. With a section on *The Measurement of Body Composition* by JOSEF BROŽEK. Springfield, Illinois: Charles C Thomas, 1960. xi, 186 pp., bibliographies, end pieces, 30 figures, 22 tables. \$5.00.

Reviewed by THEODORE D. MCCOWN, *University of California, Berkeley*

This represents the appendices A to E and the bibliography of Dr. Montagu's third edition (1960) of *An Introduction to Physical Anthropology*, given a title, repaginated, bound in a hard cover, and designed to serve as a manual for the student of anthropology. There is no index, the detailed and expanded table of contents presumably serving in its stead. The second edition of the *Introduction* contained a similar section, of 85 pages; this has now grown to 158 pages. The principal expansion involves Brožek's contribution on body composition and 23 more pages by Montagu. These include a brief disquisition on genes and the Hardy-Weinberg Law, another on "Ethnic group" and "Race," and the 1950 and 1952 UNESCO statements on Race. As material which the author could not work into the main text of his *Introduction*, they are there carried as appendices. They have quite modest relevance, if any, to the burden of the *Handbook*.

The part on "A Practical Synopsis of Methods of Measurement in Physical Anthropology" is carried over with few changes from the second edition of the *Introduction*; some of the errors are continued unchanged. One example is the method of deriving head and neck height, page 13. There is a new and wisely sensible paragraph (pp. 4-5) on the purpose of measurements. But there is no follow-up and page succeeds page with a list of measurements and their definitions with which the student will struggle. The vocabulary of the definitions is anatomical and the instructor will have to take steps to remedy that defect in knowledge of his students. The brevity of most of the definitions will also require visual-verbal reinforcing methods of demonstration and statement.

Both the somatometric and osteometric definitions of landmarks and measurements contain a high proportion of those which, in his time, Professor Hooton selected and taught as a pattern to several generations of anthropologists. But there are others which show that Dr. Montagu is, happily or unhappily, continuing the eclecticism which has characterized much anthropological description of the phenotype.

The sections on anthroposcopic and physiometric observations are pushed beyond the eclectic into the capricious and, unless the teacher has been trained by an appropriate specialist, he had better not swot it up from the seven pages devoted to blood typing technique of the living and of bone, and also the electrophoresis determinations of hemoglobins and haptoglobins.

Age at death and the sexing of skeletal and cranial material will require the instructor to do a lot of additional explanation. Epiphysial union and changes in the

pubic symphysis are ignored as age indicators and the often useful clues to sex which bones other than the skull and pelvis can provide are passed over.

Brožek's article, a tidy piece of self-contained scholarship, will be stimulating in opening up for many a new field of facts and ideas. It will pose some real problems in possible application, since most departments have neither the space nor facilities to do chemical analyses or the X-ray work described. But there may well be a rush to buy skinfold calipers. The girls and the boys in the class will enjoy pinching each other.

The experienced instructor who can pick his way through this book will find it not unhelpful in organizing his own presentation, but the inexperienced teacher and the student will find it baffling. Dr. Montagu does not make it clear why you do all the things suggested or what the result is when you do them.

*The Measure of Man: Human Factors in Design.* HENRY DREYFUSS. New York: Whitney Library of Design, 1959-1960. 16 page pamphlet with bibliography, 16 separate diagrams, 2 separate life size charts. \$4.95.

Reviewed by WILTON MARION KROGMAN, *Drexel Hill, Pennsylvania*

This tremendously synthesized and compact work depicts the American male, Joe, and the American female, Josephine. (One is reminded of Sargent's classic Norman and Norma.) The present data are anthropometrically detailed for industrial purposes, i.e., a man-machine design, with emphasis on the human size factor.

There are four detail charts showing a standing male and female, front and side views: percentiles 2.5, 50.0, 97.5. There is a table for anthropometric data for children, B-17 years; a table of basic visual data; charts of hand and foot; five charts showing male and female standing at a control board, seated at a console, and male seated in a vehicle; there are three other tables on strength and clearance, and basic display and control data.

The data, in useful and visual form, are based principally on U. S. Army studies by Hertzberg, Hooton, Randall, and on vehicular studies by McFarland. Randall's nomograph on bodily inter-correlations would have been a useful addition. This is a good example of "applied" physical anthropology and provides a useful frame of reference.

*A Drepanocitemia e a Antropologia: uma Revisão e um Estudo na Lunda e Songo.* J. H. SANTOS DAVID. (Publicações Culturais da Companhia de Diamantes de Angola No. 49.) Lisbon: Serviço de Saúde, Subsídios para o Estudo da Antropologia na Lunda, 1960. 102 pp., bibliography, 34 charts, 10 figures, 6 photographs, summaries in English, French, and Portuguese. n.p.

Reviewed by THEODOSIUS DOBZHANSKY, *Columbia University*

This elegantly, almost luxuriously, published work reports the results of examination of 5,000 native inhabitants of the districts of Lunda and Songo in Portuguese Angola for the incidence of the sickle-cell condition and of the classical O-A-B blood types. The average incidence of the sickle-cell trait is 20.5 percent, somewhat lower among persons aged 15 years or older than among younger persons. A significant, though not striking, heterogeneity is observed in different sub-districts, but no meaningful correlation with the incidence of malaria or any environmental factor is discernible.



## OTHER

*Social Statistics*. HUBERT M. BLALOCK, JR. New York: McGraw-Hill Book Co., Inc., 1960. xiv, 465 pp., appendices, chapter bibliographies, chapter glossaries, index, tables. \$7.95.

Reviewed by FRANK C. MILLER, Carleton College

*Social Statistics* is a sophisticated textbook. It should help dispel extreme points of view, both favorable and unfavorable, about the place of statistics in research on human behavior. On the one hand, its clear and not unduly technical explanations of statistical inference will give the reader a realization that statistics is not a conspiracy of the mathematically-minded to make their work inaccessible to the uninitiated and hence immune to refutation. On the other hand, it will serve to disillusion those who see in statistics an advanced kind of magic with which practitioners may reach conclusions that are otherwise elusive and thereby build a science of human behavior.

Blalock wrote this book in the belief that "it is much more important to obtain a good grasp of the fundamentals of statistical inference, together with some understanding of the assumptions underlying each test, than to master a wide range of techniques in the hope that one of them will apply to any particular problem at hand" (p. 416). Nevertheless, a wide range of techniques is covered, and by and large the exposition is good, although sometimes too condensed to be easily understood. The book is distinguished by its constant attention to fundamentals. Chapter Two is a brief but careful discussion of levels of measurement and of the role of operational definitions in the testing of hypotheses. The five chapters in Part II cover the usual topics in descriptive statistics: ratios, frequency distributions, graphs, means, deviations, and the like. Part III, "Inductive Statistics," comprises the bulk of *Social Statistics*. There are explanations of numerous techniques, including analysis of variance, partial and multiple correlation, factor analysis, sampling procedures, and many others. Of particular interest to anthropologists who apply statistical analysis to cross-cultural research are the section on Fisher's exact test for  $2 \times 2$  contingency tables with small-size  $N$ , and the discussion of the possibility of causal interpretations of correlations in certain circumstances. Throughout, the use of statistical techniques is placed within the larger framework of research, and the logic of statistical inference is explained. Other useful features of the book are the asterisks marking the most difficult passages, and the realistic and interesting exercises at the end of most chapters. The appendices containing a review of basic algebraic operations and various tables would be more useful if somewhat expanded.

*Social Statistics* is intended primarily for "those students of sociology, both advanced undergraduates and graduate students, who actually intend to engage in social research" (p. vii). As such it is a serious book, even though it largely avoids mathematical derivations and does not assume any background in college mathematics. In spite of the general excellence of this book, it is not a full preparation for the application of statistics, nor is any other book of its kind. Students who are seriously interested in quantitative methods would be well advised to start at the beginning; i.e., with the study of the mathematical foundations of statistics, including at least calculus and the theory of probability. As Blalock himself emphasizes, intelligent application depends not on "cookbook" knowledge but on a sound grasp of the principles of statistical inference; and, in the opinion of the reviewer, such principles cannot be fully understood without a familiarity with the mathematical background.

Yet this book clearly accomplishes its goal, a nonmathematical exposition of sta-

tistical inference. In anthropology, it would be particularly useful to those who wish an introduction to statistics for their own use, or to those who seek a deeper understanding of quantitative research in sociology.

*Discussions on Child Development: a Consideration of the Biological, Psychological, and Cultural Approaches to the Understanding of Human Development and Behavior.*

J. M. TANNER and BÄRBEL INHELDER (Eds.) (The Proceedings of the Fourth Meeting of the World Health Organization Study Group on the Psychobiological Development of the Child, Vol. 4, Geneva, 1956.) New York: International Universities Press Inc., 1960. xiii, 186 pp., 4 figures, index, references. \$5.00.

Reviewed by WILLIAM KESSEN, Yale University

This curious collection is something more than just another Let's-get-together-and-talk book. John Bowlby, W. Grey Walter, Konrad Lorenz, Margaret Mead, and Jean Piaget have developed, during the several years of the Study Group's activity, an apparent understanding of one another's prejudices so intimate that the reader—particularly if he does not know well the first three volumes in this series—may feel on occasion that he is intruding on a private conversation. Nonetheless, if he is willing to barge in and if he does not have an irremediable objection to disconnected discourse as an alternative to systematic presentation, he will find healthful provocation. In addition to the five headliners there were present at the meetings represented in this book eight other regular and relatively silent members of the group (F. Fremont-Smith, G. R. Hargreaves, Bärbel Inhelder, E. E. Krapf, K. A. Melin, M. Monnier, J. M. Tanner, and R. Zazzo) and two guests, Erik Erikson and L. von Bertalanffy. Erikson led a discussion of psychosexual stages, von Bertalanffy talked of General System Theory (these presentations were largely paraphrases of positions taken earlier and elsewhere by their authors), and Piaget led three sessions on equilibration, logical structures, and the definition of stages.

The report of these live meetings of the Group make up roughly the second half of *Discussions*; the rest consists of a precirculated paper by Piaget which sets a group of general questions about developmental theory, precirculated responses to Piaget's paper by seven other members of the Group, and Piaget's summation prepared after the meetings themselves. Certainly the ugliness of conference reports is not peculiar to *Discussions*, but the diffuseness of the present volume puts in specific form the broader problem—how seriously should we take the "discussions" of scholars, even as talented and prestigious as those heard in *Discussions*, who may leave behind the sobriety and polish of their usual work under the rationale of "education . . . at the truly professorial level" (p. xii)?

The fourth volume of *Discussions* is the last; the Study Group sought to bring together general principles and conclusions from their earlier, more detailed, and more heavily empirical conversations, even though they recognized the danger of flaccid overgeneralization. The book is, in fact, a contest between specific observations reluctant to general statement and laws often too inclusive to be useful. But the effort is noble and several noteworthy themes can be traced.

Chief among them, and a fair sample of the Group's work, concerned "equilibration" and the problem of stages; Piaget reaffirms his contention that the development of intelligence is directed toward "various forms of equilibrium" and that points or conditions of cognitive equilibrium in the life of the child can be seen as "stages." His colleagues chew roughly on Piaget's proposals (which are, of course, presented in detail and with examples in *Discussions*) with no one failing to make a contribution;

von Bertalanffy expresses the Group's almost unanimous distaste for "equilibrium" and proposes the substitution of "steady state of an open system," Tanner rejects with hardly a bow the utility of the notion of "stage," Grey Walter adds incisive comments about the dependence of stage-psychology on the presence of thresholds and notes the saltatory changes in the behavior of a system that may take place with a small change in input, Zazzo questions whether it is wise to look for general stages that include emotional as well as cognitive behavior, and Margaret Mead maintains that cross-cultural variation is so great as to "make any idea of general stages appear useless" (p. 49). It is not surprising in the face of this array that no clear single position about equilibration and stages (or about any other metatheoretical issue) grew from the meetings. However, there was apparently some communication and some modest revision of position; at one point Piaget is moved to say, "I have not the slightest desire to generalize from the case of logic to all the rest of mental life. Logic is the only field where equilibrium is fully achieved . . ." (p. 106), a statement greeted by Lorenz with "a sigh of relief." It is Piaget, too, who emphasizes the importance of transition rules that govern changes from one stage to another or, more generally, the nature of the mechanisms of development. It is diagnostic of the primitive state of theory in child development that this most profound of theoretical issues received hardly more than passing attention from the Study Group.

Two bits of glitter, one surely gold, may attract the special attention of anthropologists. The first is Bowlby's treatment of memory in psychoanalytic theory and the ways in which the mother may be "constructed" by the child (Bowlby's contribution is altogether superior to anything else in the book); the second is an exchange between Piaget and Mead (pp. 116-120) during which he proposes a series of cross-cultural studies of cognitive development.

The proposition can be defended that Piaget, Bowlby, and Lorenz have as much to say of consequence for an understanding of child development as any three men alive; hopefully, they will use their distillation of these discussions in future more finely finished presentations.

*The Types of the Folktale in Cuba, Puerto Rico, the Dominican Republic and Spanish South America.* TERRENCE LESLIE HANSEN. (Folklore Studies: 8.) Berkeley: University of California Press, 1957. xvi, 202 pp. \$4.50.

Reviewed by EGON SCHADEN, *University of São Paulo*

In the last decades, collections of folktales have become very numerous in Hispanic America. These texts, however, are dispersed, and the need for their classification and organization is acutely felt. The volume under review, reflecting industrious and meticulous care, is a partial answer to that need. A second volume, containing analyses of the remaining North and Central American texts will be published soon. The present study is to be understood as "part of a plan to provide a complete index of the folktale in Spain and in Hispanic America."

The classification follows the Aarne-Thompson scheme, with additions by Boggs (*Index of Spanish Folktales*, 1930), and new entries relating to animal tales by the author. What is derived from the three different sources is made clear through typography. Hansen is critical of his own classification of a certain number of tales; this obviously is to his credit.

As Aurelio M. Espinosa did, Hansen includes folktales collected among partially acculturated groups of predominant Amerindian composition. His method, however, excludes the possibility of identifying Indian or African elements. Analysis of cultural

origins and of changes undergone by tales due to peculiarities of New World environment is the remaining task for anthropologists. In this connection, Hansen's work, as most of its kind, is an indispensable research tool. Its usefulness for comparative purposes will greatly increase when similar indices of Indian myths and tales incorporated by Latin American folklore are made available.

*The Missionary's Role in Socio-Economic Betterment.* JOHN J. CONSIDINE. New York: Newman Press, 1960. xi, 330 pp., bibliography, index. \$1.75 (paperbound), \$3.75 (cloth bound).

Reviewed by H. G. BARNETT, *University of Oregon*

In the spring of 1958, 16 Catholic field missionaries, 13 nonmissionary clerical specialists, and 12 laymen met in a 4 day conference at Maryknoll Seminary in Ossining, New York, to survey socio-economic betterment in underdeveloped countries from the Catholic viewpoint. The tenor of the meetings is embodied in a statement by one of the participants, Bishop Blomjous: "One of the things we should insist upon with our missionaries is that they recognize as the real aim of missionary work not alone the making of converts but the establishment of the Church, and the Church as a Christian community. This is one of the fundamental ideas of missionary work. Once we have understood it, all that we are saying here about socio-economic betterment follows quite logically. In Africa we train our people to understand the nature of Christian society by paralleling it to their tribal society that embraces all phases of living."

Sessions on the first day were devoted to the establishment of a framework for the discussion of the topics which followed and included the alignment of the Catholic perspective on social and economic problems, a review of the community development approach to their solutions, and a definition of the role of the missionary in promoting change. The remaining sessions were focused on 12 problem areas: the family, public health, agriculture, labor, housing, rural life, urban life, leisure time, credit unions and cooperatives, small industries, women in an industrial setting, and population growth.

*The Missionary's Role in Socio-Economic Betterment* reports the proceedings of the conference. To a large extent the book consists of the editor's summary of the consensus of the several sessions, interspersed with quotations considered to be especially noteworthy. There are, however, a few sections in which prepared statements are reproduced at length. Thus, the chapter on change is devoted to summaries of the Rimrock and Homestead study by Vogt and O'Dea, the Vicos project report by Holmberg, and the analysis of the bolsa farming venture among the Papago by Dobyns. Each of these cases is measured against the four criteria of successful community development proposed by Carl C. Taylor. Another chapter presents a systematic analysis of the family and its prerequisites for continuance under the impact of a changing economy. In all problem areas there is a decided commitment to the concept of community development. As defined by the U.S. International Cooperation Administration, this is "a process of social action in which the people of a community: 1) organize themselves for planning and action; 2) define their common and individual needs and problems; 3) make group and individual plans to meet their needs and solve their problems; and 4) supplement these resources when necessary with services and materials from governmental and non-governmental agencies outside the community." As perceived by the conference, the missionary's function in the process is principally to act as its catalyst. Though he may be a technical specialist and is expected to serve as a liaison agent in contacting outside sources of help, his indispensable function is to stimulate his parish-

ioners to want desirable improvements. All of the problems considered are consequently phrased with this objective in mind. They are "missionary directed."

This orientation necessarily gives a certain character to the contents of the book. From the viewpoint of the social scientist, they are rather elementary and prosaic, but this is to be expected because they are intended to be a guide for young missionaries or for those with more experience who nonetheless have a limited acquaintance with empirical and theoretical data on cultural change. Since it is a kind of manual, the book is simply written and makes no pretense of breaking new ground. For each topic a selected bibliography is appended. There is also an addendum in which are listed the names and addresses of organizations concerned with programs of improvement in such problem areas as the family, labor, and agriculture.

Throughout the book there is an emphasis on the need for missionaries who have a systematic knowledge of the customs of the people with whom they work. The planners of the conference turned to social anthropology for a model to guide the missionary in ordering such information. The importance that they attached to this orientation is indicated by the fact that the lay participants included a sociologist and two anthropologists, one of whom presented "A Plan for the Study of a People Prior to Launching a Local Program of Socio-Economic Betterment" which amounts to a very short course on the structure and functions of a society. It is from this viewpoint that the conference and the book have significance for at least some anthropologists. That is, it documents another instance of the acceptance of their claim that anthropology can be useful as well as academic.



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## New Publications Received

- Asian Survey* (Volume I, Number I.) Berkeley: Institute of International Studies, University of California, 1961. 52 pages, notes.
- BACON, Edward. *Digging for history: archaeological discoveries throughout the world, 1945 to 1959*. Introduction by William Foxwell Albright. New York: The John Day Company, 1961. 318 pp., index, 58 plates. \$10.00.
- Barley, M. W. and A. T. Lucas (Eds.) *COWA Survey and Bibliography Area 1—British Isles*. (Series I.) Cambridge: Council for Old World Archaeology, 1958. 12 pp. (Survey), 7 pp. (Bibliography). \$1.00 the set.
- BARLEY, M. W. and A. T. Lucas (Eds.) *COWA Survey and Bibliography Area 1—British Isles*. (Series II.) Cambridge: Council for Old World Archaeology, 1960. 13 pp. (Survey), 9 pp. (Bibliography). \$1.00.
- Beardsley, Richard K. and Kwang-Chih Chang (Eds.) with Max Loehr as co-editor of Bibliography. *COWA Survey and Bibliography Area 17—Far East*. (Series I.) Cambridge: Council for Old World Archaeology, 1959. 11 pp. (Survey), 43 pp. (Bibliography). \$1.00 the set.
- Biderman, Albert D. and Herbert Zimmer (Eds.) *The manipulation of human behavior*. New York: John Wiley and Sons. 1961. xii, 323 pp., chapter references, indices. \$7.95.
- Birdsell, Joseph B. (Ed.) *COWA Survey and Bibliography Area 22—Australia*. (Series I.) Cambridge: Council for Old World Archaeology, 1959. 4 pp. (Survey), 11 pp. (Bibliography). \$1.00 the set.
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